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With the author's compl^{ts}

FRESNEL AND HIS FOLLOWERS.

A CRITICISM.

TO WHICH ARE APPENDED

OUTLINES OF THEORIES OF DIFFRACTION AND TRANSVERSAL
VIBRATION.

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TO THE

VICE-CHANCELLOR AND HEADS OF HOUSES

IN THE UNIVERSITY OF CAMBRIDGE,

THE FOLLOWING APPEAL

AGAINST THE CONTINUANCE OF A GREAT BRANCH OF THE

MATHEMATICAL COURSE PURSUED IN THAT UNIVERSITY,

IS RESPECTFULLY INSCRIBED

BY THEIR OBEDIENT SERVANT,

THE AUTHOR.

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ERRATA.

Page 44, line 2, for *xyz* read *XYZ*.

Page 56, line 5, for *evident* read *incident*.

Page 81, line 20, for δ read 8.

PREFACE.

THE circumstances under which the following pages are presented to the public are probably familiar to many of my readers. It fell to my lot to denounce a theory not merely all but universally received, but which had been made the subject of the most extravagant eulogy. In conducting such a discussion it was hardly to be expected that I should speak with very profound respect of persons who, while possessing the highest reputation for sagacity and judgment, had been seduced into patronising the most extraordinary errors. It happened however that the tone of animadversion which I felt called upon to assume did not meet with the approbation of the conductor of the periodical which I had made the medium of my communications, who, with a practical respect for authority which is sometimes found to accompany very opposite theoretical views, refused to continue the publication of my papers.

Undoubtedly, under the circumstances in which I was placed, I might have pursued a different course. I might have contented myself in the progress of my criticism with suggesting that this or that principle was doubtful, that such and such processes were unsatisfactory, that such and such conclusions were not warranted by the premises from which they were deduced. Had I done no more than this it is possible that my refutation might have been admitted to an honorable niche in the Optical Pantheon, side by side with the theories which it professed to overthrow. It is not impossible that the easy faith which could tolerate a dozen conflicting theories on the same subject at one and the same

time, might have regarded with indulgence a mild and merely speculative scepticism, which, although suggesting doubts as to every existing theory, made no very urgent call upon the adherents of any particular fallacy to renounce the worship of the false object of their admiration. This however was not the object I had in view. Fresnel's Theory appeared to me to be something more than erroneous,—it appeared to me to be absurd; and that not merely deductively, like a position in *Euclid*, but *primâ facie*, so absurd that it ought never to have been entertained. It appeared to me that, besides the injury which ordinarily results from the admission of a false theory, a grievous wrong had in this case been done to the cause of science, by the introduction of principles of the truth of which there was not a shadow of evidence, and by the allowance of processes the fallacy of which, if unsanctioned by authority, the merest tyro might have detected. It appeared to me that under these circumstances there was a call for the strongest animadversion. It was not so much my object to refute Fresnel's Theory with its train of subsidiaries, as to stigmatise the whole system of investigation which had been adopted; to shew that the methods which had been employed, not only did not but that they could not lead to a satisfactory result. I felt it to be desirable that there should be no mistake as to the nature of my views. The delusion had been too long entertained and too extensively diffused to admit of a doubtful assertion of opinion. There was too much of a disposition to put down opposition with a high hand, to allow of anything approaching to irresolution. It was necessary to give the lie direct to the Theory.

It is possible that to some of my readers Fresnel's Theory may seem less objectionable than it has done to me; it is possible that his hypotheses may appear to them less extravagant, his contradictions less palpable, his errors less gross, his contempt of the ordinary principles of reasoning less complete than I have considered them. It may be that,

although not differing from me materially in these respects, they may think the fact of having overlooked such defects less inexcusable than it has appeared to me: but holding the opinions I entertained, it was not possible but that I should express myself strongly with regard to them. It was not possible that I should comment upon principles which go to subvert the foundation of philosophical enquiry with the same composure as I might correct a mistake in arithmetic; it was not possible that I should speak of errors which I believe to be without a parallel in the history of science, as if they were of every-day occurrence and such as any one is liable to: others might have acted differently under the same circumstances—for myself I felt it would be misrepresentation to have done so.

If any one should be disposed to doubt the reality of the evils I have ascribed to the reception of Fresnel's Theory, I would recommend to his perusal the account given by Sir John Herschel (*Encyc. Met. Art. LIGHT*) of the Theory of Double Refraction; and I would suggest to him whether some singular cause must not have been in operation, when one so accomplished, so able, could be betrayed into so complete a mystification as is there exhibited.

If we look at the different theories which have been proposed as to the constitution of the ethereal medium — Fresnel's; M. Cauchy's (two); Mr. Kelland's (two); Mr. Tovey's (two at least); Sir John Lubbock's; Mr. O'Brien's (two); Professor Challis's; to say nothing of Mr. Green's, Mr. MacCullagh's, and Mr. Stokes's, every one of which possesses at this moment some more or less bewildered votaries; if I say we look at these several theories, the reflection cannot but suggest itself that those must be very odd principles of investigation which from the same results lead to such opposite causes. From the powerful machinery they have brought to bear upon the subject, one might suppose it was the object of these gentlemen to invent light rather than to discover the means already

devised and put in action for the accomplishment of that great phenomenon: nor can we be surprised if with such an aim the production of each optical Frankenstein should be some frightful monster.

Much has at various times been urged in favour of the system of trial and error, and the example of Kepler has been sufficiently enlarged upon; but I never heard it contended by the stanchest advocates of the haphazard system of philosophy, that Kepler proposed twelve curves, at one and the same time, as candidates for the honour of representing the earth's orbit. The followers of Fresnel certainly require some exponent of their system of logic. If the spirit of Newton were again to walk the earth, and could be made cognizant of the Theory of Double Refraction as originally propounded, it would surely appear to that illustrious shade that some extraordinary revolution had taken place in the laws of evidence since his hand balanced the facts of creation. The praises of Bacon have always been a favourite theme with his countrymen, occasionally I should imagine rather to the amusement of our continental neighbours, and never more conspicuously than at the present day; yet it would be difficult to conceive a more thorough repudiation of the principles of Bacon than is exhibited by the disciples of the Fresnelian Theory of Optics. The general equations of fluid motion, and the equations representing the motions of a system of attracting particles, are the magazines from which the optical theories of the present day are constructed. To devise a system of assumptions which will reduce one or other of these systems of equations to a form representing undulatory motion is the triumph of optical genius. In selecting such assumptions little attention is paid to their antecedent probability. At first, it may be, some circumspection is evinced. The first admission required of us may have some fantastical *vraisemblance*—it may be true or may be not, the chances may be about equal; but very soon this caution is lost, and in the end, provided we cannot distinctly shew that an hypothesis is untrue,

we are bound to receive it; for however profoundly convinced we may be of its having no foundation in nature, if it can be shewn "that it is not absurd nor contradictory to sound mechanical principles," (and in this respect tolerably ample latitude is allowed,) "that such *may* be true" according to Sir John Herschel, we have done something. The effect of all this is what might have been anticipated. We have after all an intellectual as well as a moral conscience; and the mental, like the moral faculty, once abused will cease to give true indications. When that is assumed as a plain fact which the mind of an angel could not compass, can we wonder if intellectual blindness should be the retribution for the intellectual sin? After all the extraordinary principles which the writers above enumerated have either invented or assented to, can we be surprised to find that each one has fallen into one or more signal mathematical errors?*

The methods adopted by Newton in the development of his great discoveries, when applied on an extended scale, ceased to be commodious, and the blind admiration which long refused to admit the defects of the machinery employed, became a serious obstacle to the progress of science in this country. It may yet be a question whether a similar consequence may not result from the indiscriminate use of the ponderous machinery of the great analysts of the last age. After all, there are other ways of apprehending truth besides through the medium of

* I am not sure that I am justified in including in the above censure M. Cauchy, as he seems rather to have failed in producing a result than in giving one which is erroneous. Of the different theories commented on in the following pages, I would here particularly call attention to that of Mr. Stokes. If the method of that gentleman is henceforth to be the standard of philosophical investigation, it will remain to us but to weep over the grave of the Inductive Philosophy. Time was when the mathematician was thought to be possessed of methods of peculiar force and efficacy, and the most commanding intellects were apt to shrink from contest with one entitled to that honourable name as from a man possessed of a keen and unerring weapon; but if the processes of Mr. Stokes are those that are to be adopted, mathematics will not only cease to be demonstrative (a favourite crotchet with some), they will cease to be reasoning. It is strange that the mystical taint which threatens religious opinion in this country should extend itself into natural philosophy: but when we find the Historian of the Inductive Sciences expressing his belief in the inspirations of genius, what can surprise us?

a differential equation. The principle of interferences was not discovered from the consideration of the equation of sound. We are still, notwithstanding all the efforts of M. Poisson and Mr. Stokes, profoundly ignorant of the general principles of molecular action, and yet we are able to solve many practical questions depending upon those principles with perfect precision. The equations of motion were not intended to supersede thought, and he must have a strange idea of the principles of philosophical inquiry who supposes that a solution of the problem of light will be most readily obtained by attempting that of a problem infinitely more general; or that the easiest mode of ascertaining a particular motion of a particular medium can be by means of an investigation involving every conceivable motion in an inconceivable number of media. It may be possible to trace in the flights of birds the vagaries of human action—to discern in the entrails of animals that which is hidden in the bosom of time—to interpret the whisperings of the Dodonæan grove into the language of articulately speaking man: but as certain as it is that no man has ever yet appeared of eye sufficiently keen or understanding sufficiently enlightened to derive any solid information from these imaginary sources of knowledge, so sure is it that no theory of light will ever be extricated from the tangled web of the general equations of motion.

In the course of the following pages I have endeavoured to point out some simple circumstances connected with undulatory motion which have not hitherto been adverted to. I have endeavoured to shew that the transversal motion which has been made the subject of such convulsive efforts, when we have divested ourselves of our ponderous harness, may not be altogether impregnable to our attacks. It had long attracted my attention, that unless some modification were made in the definition of a wave, a spherical wave consisting of transversal vibrations was a simple impossibility. If a wave be defined to be a system of similar and similarly situated laminæ

or surfaces each member of which consists of particles in the same state of motion, it is *geometrically* impossible to devise a standard according to which the particles in any spherical lamina or surface (the vibrations being transversal) can be said to be in the same phase. But although it was thus obvious that we could not describe a wave consisting of transversal vibrations as made up of elements the particles in which were in the same state of motion, there was no objection to our considering it as made up of elements the motion of the particles in which, although not identical, still possessed such a distinct and recognizable character as would enable us to speak of them as being in the same phase. It was not difficult to suggest such a modification of the ordinary definition of identity of phase as satisfied all the geometrical requirements of the problem, and the geometrical character of the motion being once distinctly apprehended, it was easy to see how it might be maintained mechanically. The mechanism I have suggested for this purpose may not be the actual mechanism, but at any rate it is one which is feasible; and without having recourse to such far-fetched illustrations as the waving of corn, the undulations on the surface of water, or the vibrations of a stretched cord, it will, unless I am greatly mistaken, render the conception of transversal vibration as familiar to the mind as that of the vibration which occurs in sound.

In applying my scheme to the explanation of polarization as it occurs in crystallized media, I was led to observe, that to whatever else might be attributable the variation of velocity in crystalline waves, it certainly was not owing to the fact of waves consisting of vibrations executed in one plane being more readily transmissible than those in which the direction of vibration is different; but, on the contrary, that the velocity is a characteristic imposed upon the wave immediately upon its entering the crystal, and that having once passed a certain narrow boundary the velocity will be maintained exactly in the same manner as the velocities of different coloured rays are main-

tained in non-crystallized media, *i. e.* irrespective of the direction of the vibration.*

Another result to which I have been conducted in the progress of these inquiries, and which may be thought of some interest, is the possibility of the occurrence of *transversal undulations* contemporaneously with and depending upon direct waves.

The theory of diffraction is presented to the reader as exhibiting results which—assuming the existence of ethereal waves—must certainly occur, and as being competent, whatever be the opinion arrived at as to its ultimate truth, to give an intelligible general explanation of the more prominent phenomena. When first studying this department of Optics as a student at Cambridge, I was led to ask myself, what became, after the diffraction, of the system of general waves which might be incident upon an aperture? That each wave must extend itself laterally was obvious, and it was not difficult to point out how this might be effected mechanically. But what would be the effect of the spreading? Would it affect the continuity of each wave? and what would be its effect on the relative positions of the waves *inter se*? It appeared to me that the continuity of each wave considered individually would not be affected, but that successive waves would vary their forms from that which they originally possessed, and that in different degrees, so that two consecutive waves intersecting at a small angle might give rise to maxima and minima of intensity. In the case of a series of plane or spherical waves advancing perpendicularly towards a circular aperture, it was easy to picture to the mind each diffracted wave converted, after passing the aperture, into a paraboloid of revolution and intersecting other waves consecutive to it in lines of annular form.

It is remarkable that a theory which explains the phenomena of diffraction with such admirable exactness as that of Dr. Young is known to do, should have completely fallen into oblivion

* As to the cause of a wave of given type being propagated uniformly in a uniform medium, and the uniform velocity of all waves in the free ether, I have arrived at certain conclusions to which I shall probably take some other occasion of giving publicity.

in this country, having been entirely superseded by the wild and erroneous scheme of Fresnel. This circumstance I conceive to be attributable to the overwhelming sense of deficiency which the early introducers of the continental mathematics into this country appear to have experienced when comparing their own attainments and capabilities with those of their neighbours; and which seems to have deprived them to a certain extent of that power of discrimination which could alone give value to their labours, and without which, in fact, the study of mathematics would be of little more utility than reading the newspapers. Sir John Herschel tells us that "Fresnel has shewn a minute though decided difference between" the places of the fringes "as given by this theory and by direct measurement"; and has moreover remarked, that were this the "true explanation, they could hardly be supposed absolutely independent of the figure of the edge of the opaque body, which experience shows they are." This may be true, but it seems to have been somewhat hastily taken for granted on the faith of a rival, and the circumstance appears the more remarkable if we compare the rejected theory with the extraordinary and unintelligible alternative which Fresnel had to propose. I have indulged in these remarks for the purpose of suggesting that had Dr. Young's theory of Diffraction been originally brought before me with due prominence, it might have been doubtful whether I should have made an attempt to devise another. As my theory however has been invented, and, as I conceive, is competent to explain the phenomena, and as the truth of the statement as to the indifference of the phenomena to the form of the edge may be true, I have thought proper to introduce the theory here, the rather as, whether true or not, as a theory of diffraction, it at any rate affords an explanation of the motion of diffracted waves—a subject to which, so far as I am aware, no attention whatever has hitherto been paid.

And now I must commend alike my theories and my criticism to the attention of the mathematical world. Many, I am aware,

more competent than myself have regarded with the same disapproval the recent theories of optics ; but either from want of sufficient interest in the subject, or from the circumstances of their position or temper, have shrunk from undertaking the thankless and dangerous duty which I have endeavoured to fulfil. I feel some love for truth and some contempt for absurdity, and I have beheld with some indignation a system of inquiry so monstrous as that of Fresnel introduced to the attention of the youthful student not merely without a warning voice as to its totally inadmissible character, but with the highest encomiums upon the success and genius of its author. The evils which have resulted from the reception of this theory are already of a sufficiently aggravated character. It has had the effect of divorcing for the time physical from mathematical science ; and great as are the advances which have of late years been made by the former in this country, they are in no degree attributable to the aid of the mathematician, but must be wholly ascribed to less pretending but more adroit and more discriminating hands. It is time that we should begin to retrace our course, and it has appeared to me highly desirable that the first step towards the reduction of the evil should be taken by that great university which has always been looked upon as the bulwark of mathematical knowledge in this country, by the interdiction for the future of a branch of study, the continuance of which can have no other effect than that of corrupting the judgment or disgusting the taste of precisely the most able and the most promising of her sons.

P.S. I cannot omit this opportunity of expressing my sense of the obligations I am under to my friend Professor Potter for the valuable assistance rendered to me upon many occasions during the preparation of the following pages, and which, from the extent and accuracy of his acquaintance with the phenomena of physical optics, is such as few persons could have afforded.

8 Mount Street, Berkeley Square,
May, 1849.

ON FRESNEL'S THEORY OF DIFFRACTION.

THE principle upon which Fresnel professes to explain the phenomena of diffraction is thus enunciated by Mr. Airy :

“The effect of any wave in disturbing any given point may be found by taking the front of the wave at any given time, dividing it into an indefinite number of small parts, considering the agitation of each of these small parts as the cause of a small wave which will disturb the given point, and finding by summation or integration the aggregate of all the disturbances of the given point produced by the small waves coming from all parts of the great wave.”

Mr. Airy offers a few words “in demonstration of this principle,” but I cannot think that he himself would consider his reasoning upon the subject to be of any peculiar force: and were the case otherwise, it would not be difficult to suggest *à priori* objections at least as cogent as Mr. Airy's *à priori* arguments. But it is not with the apparent reasonableness of this hypothesis that I have now to do, but simply with its truth, and upon this enquiry I shall at once enter.

The hypothesis in question is thus applied by Sir John Herschel to the investigation of the leading experiment in the theory of Diffraction, viz. that in which spherical waves are diffracted at a single edge. (*Encyc. Met. Art. LIGHT*, 718.)

“Let us consider a wave *AMF* (fig. 1) propagated from *O*, and of which all that part to the right of *A* is intercepted by an opaque body *AG*; and let us consider a point *P* in a screen at the distance *AB* behind *A* as illuminated by the undulations emanating simultaneously from every point of the portion *AMF* according to the theory laid down in Art. 628, *et seq.* For simplicity, let us consider only the propagation of undulations

in one plane. Put $AO = a$, $AB = b$, and suppose $\lambda =$ the length of an undulation; and drawing PN any line from P to a point near M , put $PN = f$, $NM = s$, $PB = x$; then supposing P very near to B , and with centre P , radius PM describing the circle QM , we shall have

$$f = PQ + QN = \sqrt{\{(a+b)^2 + x^2\}} - a + QN = b + \frac{x^2}{2(a+b)} + QN.$$

Now QN is the sum of the versed sines of the arc s to radii OM and PM , and is therefore equal to

$$\frac{s^2}{2OM} + \frac{s^2}{2PM} = \frac{s^2}{2} \left(\frac{1}{a} + \frac{1}{b} \right) = \frac{a+b}{2ab} s^2;$$

so that finally

$$f = b + \frac{x^2}{2(a+b)} + \frac{a+b}{2ab} s^2.$$

Now if we recur to the general expression in Art. 632, for the motion propagated to P from any limited portion of a wave, we shall have in this case $a\phi(\theta) = 1$, because we may regard the obliquity of all the undulations from the whole of the efficacious part of the surface AMN as very trifling, when P is very distant from A in comparison of the length of an undulation; and as we are now considering undulations propagated in one plane, that expression becomes merely

$$V = \int ds \sin 2\pi \left(\frac{t}{T} - \frac{f}{\lambda} \right),$$

and the corresponding expression for the excursions of a vibrating molecule at P will be

$$X = \int ds \cos 2\pi \left(\frac{t}{T} - \frac{f}{\lambda} \right).$$

If then we put for f its value, and take

$$2\pi \left\{ \frac{t}{T} - \frac{b}{\lambda} - \frac{x^2}{2\lambda(a+b)} \right\} = \theta, \quad s \sqrt{\left\{ \frac{2(a+b)}{ab\lambda} \right\}} = \gamma,$$

and consider that in those expressions t and x remain constant while s only varies, the latter will take the form

$$X = \sqrt{\left\{ \frac{ab\lambda}{2(a+b)} \right\}} \left\{ \cos \theta \int dr \cos \left(\frac{\pi}{2} r^2 \right) + \sin \theta \int dr \sin \left(\frac{\pi}{2} r^2 \right) \right\};$$

which shews that the total wave on arriving at P may be regarded as the resultant of two waves, $x' \cos \theta$ and $x'' \sin \theta$, differing in their origin by a quarter undulation, and whose amplitudes x' and x'' are given by the expression

$$X' = \sqrt{\left\{ \frac{ab\lambda}{2(a+b)} \right\}} \int dr \cos \left(\frac{\pi}{2} \gamma^2 \right),$$

$$X'' = \sqrt{\left\{ \frac{ab\lambda}{2(a+b)} \right\}} \int dr \sin \left(\frac{\pi}{2} \gamma^2 \right),$$

the integrals being taken between the limits of γ corresponding to $s = -AM$ and $s = +\infty$.

It is obvious that if the principle of secondary waves can be applied to a problem like the above, it may also be applied to find the illumination at any point in front of a wave when no diffraction takes place. In this case, taking the integrals X' , X'' between the limits $+\infty$ and $-\infty$, we obtain

$$\begin{aligned} X &= \sqrt{\left\{ \frac{ab\lambda}{2(a+b)} \right\}} \cdot (\cos \theta + \sin \theta) = \sqrt{\left(\frac{ab\lambda}{a+b} \right)} \cdot \cos (\theta - 45^\circ) \\ &= \sqrt{\frac{ab\lambda}{a+b}} \cos 2\pi \left\{ \frac{t}{T} - \frac{b}{\lambda} - \frac{x^2}{2\lambda(a+b)} - \frac{1}{8} \right\}, \end{aligned}$$

substituting for θ . It is evident from this result that the phase of vibration will be the same for all points at which $b + \frac{x^2}{2(a+b)}$ is constant, where x and $a+b$ are the coordinates measured in the plane of the paper from 0 of the point whose vibration is considered. But it is clear that where there is no diffraction the phase must be the same for all points at which $(a+b)^2 + x^2$ is constant, since the waves are by hypothesis spherical. Hence it appears that the hypothesis of secondary waves gives us a false result in this case.

But it may possibly be urged that in the case of diffraction at a single edge there are peculiar circumstances operating, such as the obliquity of emanation of the secondary waves, which we cannot take accurately into account; and that to these the failure of the method in the above instance may be attributable. I shall not stop to enlarge upon the imperfection

of a theory of diffraction which fails to explain a phenomenon which has always been regarded as the touchstone of the subject, but shall proceed to examine whether in the case of narrow apertures any greater reliance is to be placed in the principle of secondary waves than in that which we have been considering.

Let AB (fig. 1') be the aperture, and let OP bisect it. With centre O describe the circles AEB , aeb indefinitely near to each other. AB will then represent the front from which the small waves are supposed to emanate in the ordinary theory. It is obvious that if we draw Ac , Bd touching the inner circle in b and d , we may consider the small waves to emanate from $cabd$, and the illumination at P obtained upon this hypothesis ought, if the theory were true, to be identical with that obtained upon the hypothesis of AB being the origin of emanation.

In estimating the effect at P of the small waves from $cabd$, it will be necessary to ascertain the effect upon the latter of the diffraction which they will necessarily undergo at A and B . For this purpose, produce each side of the aperture to meet the inner wave in a and b respectively, then it is evident that the effect upon P of the small wave from any point of $cabd$ which lies between a and b , will be the same as if no opaque body intervened, since in the case of any such wave the angle between the lines which join the centre of emanation with the diffracting edge and the point P , whose illumination is being considered respectively, will be greater than a right angle.

I shall now shew that the illumination at P arising from the small waves from ac , bd , will be of the order δ^2 , where $\delta = OE - Oe$.

Referring to fig. 1, it will be seen that if a be the radius of a wave emanating from a luminous point O , and which touches the diffracting edge, f the distance of any point N in the wave from the point P in front of it, s the distance measured along the front of N from the point where the wave is intersected by OP , the illumination I at P due to the wave from N will be

$$(1) \quad = \int ds \cos \frac{2\pi}{\lambda} (rt - f)$$

between the proper limits; where

$$(1) \quad f = \sqrt{\left\{ (a+r)^2 + a^2 - 2a(a+r) \cos \frac{s}{a} \right\}} \\ = r + a \left(1 - \cos \frac{s}{a} \right),$$

if a be indefinitely small;

$$\therefore I = \int \left\{ ds \cos \frac{2\pi}{\lambda} (vt - r - a) - \frac{2\pi}{\lambda} a \cos \frac{s}{a} \sin \frac{2\pi}{\lambda} (vt - r - a) \right\} \\ = s \cos \frac{2\pi}{\lambda} (vt - r - a),$$

neglecting squares of a : or if s' be the difference between the limiting values of s ,

$$I = s' \cos \frac{2\pi}{\lambda} (vt - r),$$

where s' is of the same order as δ . Hence if we now take s to represent the arc $cabd$ in fig. 1, the illumination at P due to the small waves from ac may be represented by

$$\Sigma(I) = s' \int ds \cos \frac{2\pi}{\lambda} (vt - r);$$

and since ac can be diminished indefinitely by diminishing δ , it follows that $\int ds \cos \frac{2\pi}{\lambda} (vt - r)$ can be diminished indefinitely, and $\Sigma(I)$ will be of the order δ^2 .

If we now apply the formulæ (1) to find the illumination x at P , produced by the small waves from aeb , it is evident that we shall have

$$X = \int ds \cos \frac{2\pi}{\lambda} (vt - f_1),$$

where $f_1 = \left\{ (a_1 + r_1)^2 + a_1^2 - 2a_1(a_1 + r_1) \cos \frac{s}{a_1} \right\},$

the limits of ρ being ea and $-eb$ (e_1 and $-e_1$ suppose); where $a_1 = a - \delta$ and $r_1 = a + \delta$.

If s be small $f_1 = r_1 + \frac{a_1 + r_1}{2a_1r_1} s^2,$

and $X = \int_{-e_1}^{+e_1} ds \cos \frac{2\pi}{\lambda} \left(vt - r_1 - \frac{a_1 + r_1}{2a_1r_1} s^2 \right).$

But if we had considered the emanation to take place from

AB instead of ab , we should in the same manner have found

$$X = \int_{-e}^{+e} ds \cos \frac{2\pi}{\lambda} \left(vt - r - \frac{a+r}{2ar} s^2 \right),$$

where $e = AE$; and comparing these two values of X it is obvious that we must have

$$\int_{-e_1}^{+e_1} ds \frac{\sin}{\cos} \left(\frac{2\pi}{\lambda} \cdot \frac{a_1 + r_1}{2a_1 r_1} s^2 \right) = \int_{-e}^{+e} ds \frac{\sin}{\cos} \left(\frac{2\pi}{\lambda} \frac{a+r}{2ar} s^2 \right),$$

and therefore
$$\int_0^{e_1} ds \sin \frac{\pi}{\lambda} \frac{a_1 + r_1}{a_1 r_1} s^2 = \int_0^e ds \sin \frac{\pi}{\lambda} \frac{a+r}{ar} s^2,$$

an equation which holds accurately so long as we confine ourselves to the first powers of δ .

Putting $\phi(e)$ for the second member of the last equation, it may easily be shewn that

$$\int_0^{e_1} \sin \frac{\pi}{\lambda} \frac{a_1 + r_1}{a_1 r_1} s^2 = \sqrt{\left(\frac{a_1 r_1}{ar} \right)} \phi \left\{ \sqrt{\left(\frac{ar}{a_1 r_1} \right)} e_1 \right\};$$

therefore
$$\phi(e) = \sqrt{\left(\frac{a_1 r_1}{ar} \right)} \phi \left\{ \sqrt{\left(\frac{ar}{a_1 r_1} \right)} e_1 \right\} \dots (2).$$

Now
$$\sqrt{\left(\frac{a_1 r_1}{ar} \right)} = 1 + \frac{\delta}{2} \left(\frac{1}{r} - \frac{1}{a} \right),$$

and
$$e_1 = e - Aa = e - \frac{\delta}{\sin \frac{e}{a}}.$$

Substituting these values in (2) we get

$$\phi(e) = \left\{ 1 + \frac{\delta}{2} \left(\frac{1}{r} - \frac{1}{a} \right) \right\} \phi \left\{ e - \delta \sec \frac{e}{a} - \frac{\delta e}{2} \left(\frac{1}{r} - \frac{1}{a} \right) \right\},$$

whence, expanding the right-hand member of the equation, and equating to zero the coefficient of δ , we obtain

$$\left(\frac{1}{r} - \frac{1}{a} \right) \phi(e) = \left\{ \left(\frac{1}{r} - \frac{1}{a} \right) e + 2 \operatorname{cosec} \frac{e}{a} \right\} \phi'(e),$$

an equation which is not satisfied by

$$\phi(e) = \int_0^e \sin \frac{\pi}{\lambda} \frac{a+r}{ar} \rho^2.$$

Hence in the case of narrow apertures equally as in the case of diffraction at a single edge, the hypothesis of secondary waves leads to inconsistent results. It is obvious therefore that the theory depending upon that hypothesis must be entirely rejected.

A NEW THEORY OF DIFFRACTION.

(Altered from the *Philosophical Magazine*, Vol. xxvii. p. 46.)

IN a former paper I endeavoured to point out the fallacy of the celebrated hypothesis devised by Fresnel for the explanation of the phenomena of Diffraction. I endeavoured to shew that when that principle is applied to the investigation of facts which are simple and ascertainable, it gives results which are inconsistent with truth; and that when it is applied to the explanation of those phenomena which it is the object of every theory of diffraction to elucidate, it leads to conclusions which are inconsistent with each other. I did not attempt to refute the evidence which may have been brought forward in support of this principle, simply because I found none. Those who are desirous of ascertaining Mr. Airy's very vague sentiments with regard to this subject, will find them embodied in five lines of p. 267 of his *Mathematical Tracts*. Sir John Herschel appears to have considered that the principle of secondary waves was "either self-evident or followed immediately from the elementary principles of dynamics;" but he nowhere shews us how it follows from those principles, nor have I heard of any of his successors having ventured upon so desperate an attempt.*

But though entertaining a strong opinion as to the absurdity of the principle of secondary waves, I am desirous that the scope of these observations should not be misunderstood. The principle in question has been represented by some leading writers on this subject as so completely part and parcel of the undulatory theory, that the student who is little conversant

* The hypothesis of secondary waves seems to bear considerable resemblance to the ancient astronomical theory of solid epicycles—as wild, and as fallacious.

with these subjects might suppose that by rejecting it we reject the theory *in toto*, but such is by no means the case. The principle is quite collateral to the theory. With the investigations into the phenomena of polarized light, it has absolutely no connexion whatever. Rejecting it as I do, I still admit and profess my belief in the truth of the undulatory theory of reflexion and refraction, of the explanations of the interesting experiments of the two mirrors and the prism of a small refracting angle, and of that admirable portion of the theory which relates to the phenomena observed in the shadows of thin fibres and the colours of Newton's rings. But if there be any soundness in the principles I have laid down, the remaining portion of the theory of diffraction, the whole theory of apertures and of the shadows of extended bodies must be entirely rejected.

Although, however, I thus feel constrained to shew the futility of the received theory of diffraction, I do not consider that the elucidation of this class of phenomena upon undulatory principles is by any means to be despaired of. With regard to Dr. Young's hypothesis, which attributes the fringes about the edge of the shadow of an extended body to the interference of the direct light with the small portion reflected from the edge of the diffracting body, I have indeed little to say. It has been very positively asserted that the breadth of the fringes is independent of the nature of the diffracting body and of the form of the edge, a fact which if established would form an insuperable objection to the reception of Dr. Young's hypothesis. It may be shewn, however, that the phenomena of diffraction may be accounted for on the simplest principles, and in a manner entirely free from this objection.

Suppose a spherical wave, diverging from a point, to consist of one condensation and one rarefaction. (*Vide* fig. 2.)

Let O be the point of divergence, $A'B'A''B''$ the condensed, $ABAB'$ the rarefied portion of the wave after it has been diffracted by the opaque body AC . It is obvious that the portion $AB'A''B''$, in which the density of the ether is greater than that of rest, will tend to relieve itself by flowing in the direction $B''A''$ into the geometrical shadow, (in which the ether has the

mean density); while on the other hand, as the density in $AB A'B'$ is less than that of rest, the ether will tend to flow in the direction $A'B'$ out of the geometrical shadow into the portion $AB A'B'$. Hence the condensation $A'B' A''B''$ will be prolonged into the geometrical shadow and likewise the rarefaction $AB A'B'$, or in other words the wave itself will be prolonged within the shadow.

If we now examine into the effect of this result (which it will be observed is the *inevitable consequence* of the diffraction) upon the constitution of the wave, it will be obvious in the first place that there will be no discontinuity, *i.e.* the wave will be continuous up to the point where it finally terminates. We have here considered light as consisting of waves of which the vibrations are executed in the direction of transmission, but the same would be the case whatever were the nature of the vibration; for it is certain that the waves have the power of diffusing themselves within the geometrical shadow; and if we lay aside the consideration of any disturbing effect produced by the forces inherent in the diffracting body, the non-existence of which we are now assuming to be established upon experimental grounds, it seems inconceivable that this effect should be operated otherwise than continuously.

This consideration, if just, is of the highest importance, for it shews us that if the phenomena of diffraction are to be accounted for at all upon undulatory principles, and if it is at the same time to be considered as established, that those phenomena are entirely independent of the specific action of the diffracting body, there is but one way left in which to look for an explanation. If each wave taken singly tends to preserve its continuity, interference is impossible except through the intersection of consecutive waves nearly parallel to each other.

Now, in order to the intersection of which we are here speaking, it is obvious that the waves which before diffraction are concentric and spherical, must, after diffraction, vary from the spherical form, and that too in different degrees, in order to enable a second wave to overtake so as to intersect its immediate predecessor. That such a change of form should take place is not at all improbable. We are accustomed indeed to consider

the velocity of waves traversing a uniform medium as invariable; but the waves to which attention has hitherto been directed, have always been either continuous or *quasi* continuous, *i. e.* they have either been supposed to be spherical or to be propagated along the axis of a cone or cylinder. But the case of broken waves is strikingly different.

Suppose a plane wave to traverse a cylindrical tube having its front perpendicular to the axis. If a portion of the side of the tube towards its extremity were cut away, it is obvious that when the wave arrives at this point it will no longer retain its form of a plane surface, but the part of it which is no longer supported by the side of the tube will be weakened by diffusion, and will fall behind the position it would have held had the tube been preserved intact. This is precisely the case of the experiment in diffraction which we are now considering.

We are told indeed that in the free ether all disturbances are propagated with the same velocity, but, as I have before suggested, it is impossible to reason strictly from the case of continuous waves to that of the waves which we are now considering, which, besides being subjected to all the circumstances which affect the former, have this further peculiarity of being subject to a lateral infusion and effusion of particles. In the one case the effect of the wave is exerted only on the particles in its front; in the other the motion is not only communicated forwards, but also laterally. It is certain that the part of the wave which has extended itself *within* the geometrical shadow is *not* circular; for if it were it would obviously be impossible that any interference should take place within the geometrical shadow of a narrow fibre: and if the part of the wave *within* the geometrical shadow deviate from the spherical form, is it very incredible that the part *without* the shadow, immediately contiguous to the former, should suffer a similar deviation?

Assuming then that a wave, after diffraction, may undergo such change of form as I have described, and to such an extent as to allow of its overtaking its immediate predecessor, it is easy to see how the intersection of the two, when nearly parallel to each other, should produce interference.

Let ABB_2A_2 , CDD_2C_2 (Fig. 3) represent portions of two waves intersecting each other at a small angle ABB_1A_1 ; CDD_1C_1 respectively representing the condensed portions of the waves $A_1B_1B_2A_2$; $C_1D_1D_2C_2$ the rarefied portions. A very little consideration will serve to shew that the result of the intersection must be to give us a bright point in the centre with a dark one on each side. As the waves move on, since their relative change of form will be very slow, the locus of their intersection will describe a curve differing slightly from a straight line; and as the relative change of form goes on diminishing as the wave advances, it is obvious that the path described will bear a general resemblance to an hyperbola which lies very near to its asymptote. The interference I have described will, it is true, take place at a finite distance from the edge of the diffracting body, but it is easily conceivable that this distance may be so small as to be inappreciable to the senses. If we suppose again that the original wave is intersected in a similar manner by its *second* consecutive wave, it is obvious that a second line of interference will be formed which will lie without the former; for they will both have the same sensible origin, and the relative variation of form in the last case will obviously be greater than in the former, so that the locus of the intersection will tend to recede further from the geometrical shadow. The existence of additional lines of interference may be accounted for in a similar manner, and we are thus led to a new and further analysis of the composition of light, which is this, that in ordinary cases, *i.e.* when the light is steady, what is commonly called homogeneous light (meaning thereby light of one colour) really consists of a number of different waves which occur in the same invariable cycle, the members of the cycle being so adjusted with regard to each other as to produce the effects I have above endeavoured to describe.

It may seem strange that waves which, when subjected to the test of refraction, evince no difference of velocity, should, when tried by another standard exhibit so remarkable a diversity; but when we consider the vast variety of waves which are supposed to traverse the free ether with the same velocity, it is not unnatural to imagine that even those which traverse a

given medium with the same velocity may in reality comprise a variety of types.

That such a systematic adjustment as that which I have supposed to exist among the waves composing homogeneous light should occur, need not be thought incredible when we reflect that, the emission of the various coloured waves which produce the solar spectrum must to a certain extent occur in an invariable order, or otherwise we should have the relative brightness of the different parts of the spectrum varying from one moment to another, which there is no reason to suppose is the case. It is true that the eye is not a very critical judge in such matters; and therefore the uniformity of emission which the apparent absence of change in the intensity of the different parts of the solar spectrum appears to indicate, may not after all be so accurate as one might at first suppose. But whether this uniformity be, in point of fact, accurate, or only approximate, it is just as easy to conceive it the one as the other; in other words, it is not more inconceivable that the order of emission of the different coloured rays should be invariable, than that its deviations should be so small as not to be discoverable by the senses. Hence the hypothesis as to the accurate adjustment of the waves composing light of one colour, which I have supposed, hardly presents more difficulty than the theory of colours.

But whatever be thought of the probability of this hypothesis, it is obvious that, if we are to consider the objections to Dr. Young's theory as established, our choice lies almost between this and none. Before we allow ourselves to be driven to the conclusion that the phenomena of diffraction are to be attributed to the action of the diffracting body on the waves taken separately, rather than to the mutual action of the waves upon one another, we shall do well to reflect upon the extreme improbability, which upon a popular view of the matter there seems, of such being the case, and of the refined nature of the considerations which would be requisite in order to expose by accurate methods the fallacy of such popular impressions, and let us then judge of the likelihood of such an investigation being conducted to a satisfactory issue. Moreover, if the waves individually possess the faculty, when diffracted, of breaking up

into parts, how does it happen that this only occurs without the geometrical shadow? If any portion of the wave were broken into fragments, we should surely expect that it would be that which has been newly created, namely that within the geometrical shadow; yet we know that in this direction nothing of the kind takes place.

But it is time that I should commend my theory, such as it is, to the consideration of my readers. If I have dealt severely with the theories of another, it has not been simply because I believed them to be erroneous, but because I considered them to involve a total disregard of all sound principles of argument. Of the extraordinary vagaries in which Fresnel was in the habit of indulging, we have a sufficient specimen in his theory of diffraction, which I have considered in my first paper; and of which other examples equally remarkable will be given in the subsequent portion of this work. The mere fact of such views having been published and not contradicted might not have induced me to take, with regard to them, the course I have adopted; but when they are not merely passed over in silence, or with approval, by the highest authorities, but have likewise obtained for their author one of the greatest reputations of modern times, I thought it was time to speak out. The theory which I have proposed may not be true, but, unless self-love deceives me, it contains nothing that is very improbable or which cannot be readily understood. When I say that Fresnel's theory both of polarized and unpolarized light is liable to the severest censure upon both of these grounds, I hope I shall be considered to have offered a sufficient apology for claiming a leniency of consideration in my own case, which I have not thought proper to extend to him.

Berne, August 17, 1846.

ON FRESNEL'S THEORY OF DOUBLE REFRACTION.

(From the *Philosophical Magazine*, Vol. xxvii. p. 553.)

HAVING on several previous occasions treated of the theory of unpolarized light, and having as I trust effectually exposed the futility of the celebrated hypothesis devised by Fresnel for the elucidation of many of the principal phenomena in that department of Optics, I now come to the consideration of the subject of polarized light; upon this treatment of which, Fresnel's great fame now principally rests, and to whose views in regard to which many of his adherents, who have felt themselves compelled to give up his theory of diffraction, still cling with unshaken fidelity. What my own faith in this respect may be, it is unnecessary at present to disclose further than this, that whether the original idea of transversal vibrations is due to Young only or not, I can pledge myself to prove that the researches of Fresnel have not advanced us one step beyond it.

Fresnel's first step in the mathematical part of his theory is to prove the existence of the axes of elasticity, "a proposition," says Mr. Smith (*Cambridge Mathematical Journal*, Vol. i. p. 1), "on which the whole theory of double refraction depends, and which Fresnel has proved by a method which has the advantage of geometrical distinctness, but which is long and rather difficult to follow out on that account." On the same account I shall prefer to give Mr. Smith's elegant analysis, instead of the cumbrous processes of Fresnel, trusting that my readers will take my word for it, that whatever it may want in "geometrical distinctness" it gains in logical clearness.

"The proposition is thus stated:—In every system of particles acting on each other with forces which are functions of their mutual distances, there are three directions at right angles to each other, along which if a particle be displaced, the forces of restitution will act in the same direction.

“ Let xyz be the coordinates of the attracted, $x_1y_1z_1, x_2y_2z_2\dots$ those of the attracting points, $r_1r_2r_3\dots$ the distances between the attracted and attracting points, $\phi_1(r_1) \cdot \phi_2(r_2) \cdot \phi_3(r_3)\dots$ the attractions; XYZ the total resolved forces along the axes; then we shall have

$$X = \frac{x_1 - x}{r_1} \phi_1(r_1) + \frac{x_2 - x}{r_2} \phi_2(r_2) + \&c.,$$

and similarly for Y and Z . Now let

$$R = - \Sigma \int \phi(r) dr,$$

$$\text{then } X = \frac{dR}{dx} = 0,$$

$$Y = \frac{dR}{dy} = 0,$$

$$Z = \frac{dR}{dz} = 0,$$

when the particle is in equilibrio.

“ Let the particle receive a small displacement, the projections of which on the coordinate axes are $\delta x, \delta y, \delta z$. Then, supposing the displacement to be very small, the force of restitution may be taken as proportional to it, so that we have

$$\left. \begin{aligned} X &= \frac{d^2R}{dx^2} \delta x + \frac{d^2R}{dx dy} \delta y + \frac{d^2R}{dx dz} \delta z, \\ Y &= \frac{d^2R}{dx dy} \delta x + \frac{d^2R}{dy^2} \delta y + \frac{d^2R}{dy dz} \delta z, \\ Z &= \frac{d^2R}{dx dz} \delta x + \frac{d^2R}{dy dz} \delta y + \frac{d^2R}{dz^2} \delta z, \end{aligned} \right\} \dots\dots\dots (A).$$

“ Now the force of restitution will be in the direction of the displacement if XYZ be proportional to $\delta x, \delta y, \delta z$.

$$\text{Let then } \rho = \frac{X}{\delta x} = \frac{Y}{\delta y} = \frac{Z}{\delta z};$$

$$\text{then putting } \frac{d^2R}{dx^2} = A, \quad \frac{d^2R}{dy^2} = B, \quad \frac{d^2R}{dz^2} = C,$$

$$\frac{d^2R}{dz dy} = A', \quad \frac{d^2R}{dz dx} = B', \quad \frac{d^2R}{dx dy} = C',$$

and, substituting in the former equations, they become

$$\begin{aligned}(A - \rho) \delta x + C' \delta y + B' \delta z &= 0, \\ C' \delta x + (B - \rho) \delta y + A' \delta z &= 0, \\ B' \delta x + A' \delta y + (C' - \rho) \delta z &= 0.\end{aligned}$$

From which it is easy to prove, supposing the above process correct, "that there are three directions at right angles to each other, along which if a particle be displaced, the force of restitution acts in the same direction."

But the fact is, the above process is entirely fallacious, if it is meant to apply to the case of the motion of a particle of the ethereal medium by which light is produced. What is meant by the mysterious principle that, supposing the displacement to be very small, the force of restitution may be taken as proportional to it," I confess myself unable to comprehend: but this I do understand, that when the coordinates of the particle whose motion is being considered vary from xyz to $x + \delta x, y + \delta y, z + \delta z$, the coordinates of the other particles of the medium will vary from $x_1 y_1 z_1, x_2 y_2 z_2$, &c. to $x_1 + \delta x_1, y_1 + \delta y_1, z_1 + \delta z_1, x_2 + \delta x_2, y_2 + \delta y_2, z_2 + \delta z_2$, &c., and that therefore the above values (A) for the resolved parts of the force on the particle whose motion is being considered are entirely fallacious.

The true value of X in this case is

$$\begin{aligned}& \frac{d^2 R}{dx^2} \delta x + \frac{d^2 R}{dx dy} \delta y + \frac{d^2 R}{dx dz} \delta z, \\ & + \frac{d^2 R}{dx dx_1} \delta x_1 + \frac{d^2 R}{dx dy_1} \delta y_1 + \frac{d^2 R}{dx dz_1} \delta z_1, \\ & + \frac{d^2 R}{dx dx_2} \delta x_2 + \frac{d^2 R}{dx dy_2} \delta y_2 + \frac{d^2 R}{dx dz_2} \delta z_2 + \&c.,\end{aligned}$$

and similarly for Y and Z : from which it is evident, that to talk of the existence of axes of elasticity in every system of particles acting on each other with forces varying according to their mutual distances is mere absurdity. And hence it appears that the "proposition on which the whole theory of double refraction depends" is entirely untrue.

Will it be urged, however, that although the general proposition does not hold, there still may be particular systems of

particles for which it does hold? I do not hesitate to state my belief that the existence of such a system is *impossible*, and at any rate would challenge any analyst whatever to suggest any such.

The case then stands thus: a writer states a proposition as the basis of a theory; he offers a proof of the proposition which turns out to be fallacious; and not only is the proof itself erroneous, but during the investigation there appears a degree of evidence approaching to certainty, that the proposition itself, after modifying it in every conceivable way consistent with the circumstances of the case to which it is meant to apply, is untrue; and there is moreover a *perfect certainty* that it is incapable of proof. Thus we have a fundamental proposition of which a false proof is given, a certainty that if true it must always remain a mere assumption incapable of independent proof; and this in the face of the fact that there is every reason to suppose it untrue. Such a combination of circumstances would have decided the fate of any other theory; why is this to be made an exception to the rule? But to return.

Assuming the existence of the axes of elasticity, we are next introduced to the surface of elasticity. Referring the co-ordinates to the axes of elasticity, we have

$$\left. \begin{aligned} X &= a\delta x = ar \cos \alpha \\ Y &= b\delta y = br \cos \beta \\ Z &= c\delta z = cr \cos \gamma \end{aligned} \right\} \text{where } a, b, c \text{ are constants.}$$

The rest I shall give in the words of Sir John Herschel, (vide *Encyc. Met. Art. LIGHT*, 1004).

“ M. Fresnel next conceives a surface, which he terms the ‘surface of elasticity,’ constructed according to the following law: On each of the axes of elasticity, and on every radius r drawn in all directions, take a length proportional to the square root of the elasticity exerted on a displaced molecule by the medium in the direction of the radius or to $\sqrt{F}$: then if we call R this length, or the radius vector of the surface of elasticity, we shall have

$$R^2 = \{ ar \cdot (\cos \alpha)^2 + br \cdot (\cos \beta)^2 + cr \cdot (\cos \gamma)^2 \} \times \text{const.},$$

the values of R parallel to the axes are then had by the equations

$$R^2 = \text{const.} \times ar, \quad R^2 = \text{const.} \times br, \quad R^2 = \text{const.} \times cr,$$

which we shall express simply as a^2, b^2, c^2 , so that the equation of the surface of elasticity will be of the form

$$R^2 = a^2 \cos^2 X + b^2 \cos^2 Y + c^2 \cos^2 Z,$$

where X, Y, Z now stand for α, β, γ , the angles made by R with the axes of co-ordinates.

“Let us now imagine a molecule displaced and allowed to vibrate in the direction of the radius R , and retained in that direction, or at least let us neglect all that portion of its motion which takes place at right angles to the radius vector. Then the force of elasticity by which its vibrations are governed will be proportional to R^2 , and the velocity of the luminous wave propagated by means of them, in a direction transverse to them (or at right angles to R), will be proportional to R .”

Of this extraordinary proposition the accomplished author does not offer one syllable of proof or explanation. Whether Fresnel's writings are equally deficient I am not aware; but another eminent mathematical writer, the present Astronomer Royal, after bestowing, as we may reasonably suppose, some degree of care on the study of Fresnel's papers, appears to have found nothing better in the way of demonstration than the following, (*vide* Airy's *Tracts*, 2nd edit., p. 341):

“To explain on mechanical principles the transmission of a wave in which the vibrations are transverse to the direction of its motion.

“In fig. (4) let the faint dots represent the original situations of the particles of a medium, arranged regularly in square order, each line being at the distance h from the next. Suppose all the particles in each vertical line disturbed vertically by the same quantity, the disturbance of different vertical lines being different. Let x be the horizontal abscissa of the second row, $x - h$ that of the first, and $x + h$ that of the third; let u, u_1 , and u' be the corresponding disturbances. The motions will depend upon the extent to which we suppose the forces are sensible. Suppose the only particles, whose forces on A are sensible, to be $BCDEFG$ (omitting those in the same line as their attractions are equal and in opposite directions); and suppose them to be attractive, and as the inverse square of the distance; and

the absolute force of each = m . The whole force to pull A downwards is

$$\begin{aligned} & \frac{m(h+u-u_1)}{\{h^2+(h+u-u_1)^2\}^{\frac{3}{2}}} + \frac{m(u-u_1)}{\{h^2+(u-u_1)^2\}^{\frac{3}{2}}} \\ & - \frac{m(h-u+u_1)}{\{h^2+(h-u+u_1)^2\}^{\frac{3}{2}}} + \frac{m(h+u-u')}{\{h^2+(h-u-u')^2\}^{\frac{3}{2}}} \\ & + \frac{m(u-u')}{\{h^2+(u-u')^2\}^{\frac{3}{2}}} - \frac{m(h-u+u')}{\{h^2+(h-u+u')^2\}^{\frac{3}{2}}}. \end{aligned}$$

Expanding these fractions and rejecting powers of $u-u_1$, and $u-u'$ above the first, the force tending to diminish u is

$$\left(1 - \frac{1}{2}\right) \frac{m}{h^3} (2u - u_1 - u').$$

Putting for u_1 , $u - \frac{du}{dx} \cdot h + \frac{d^2u}{dx^2} \frac{h^2}{1.2}$,

and for u' , $u + \frac{du}{dx} h + \frac{d^2u}{dx^2} \frac{h^2}{1.2}$;

we find $\frac{d^2u}{dt^2} = \left(1 - \frac{1}{2}\right) \frac{m}{h} \frac{d^2u}{dx^2}$,

an equation of exactly the same form as that for the transmission of sound. The solution therefore has the same form; and therefore the transversal motion of particles supposed here follows the same law, that is, it follows the law of undulation." And, moreover, if the above were correct, the velocity of the luminous wave would be proportional to the square root of the force of elasticity in a direction transverse to the direction of the course of the wave.

Whether the above illustration, if indeed it may be called such, is due to Fresnel, or to Mr. Airy himself, I am not aware; but if to the latter, it is plain that Fresnel's mantle has fallen upon him. The whole is erroneous from beginning to end. Not only do the mathematics fail to meet the case under consideration, but there is a palpable mathematical error in the process, which, even admitting the data, completely vitiates the result.

I need but to advert to the circumstance, that in the approximate values substituted for u_1 and u' , it is assumed that h is small with respect to u , or that the distances between the particles are small compared with their actual motions, a supposition entirely at variance with the assumed data of the problem. Hence it is plain that this supposed illustration is for every purpose entirely worthless.

Thus as we were compelled to assume the existence of the axes of elasticity, not only in default, but in the face of evidence, so we are compelled to assume the rule as to the mode of calculating the velocity on a bare analogy to a case presenting the most striking difference from that under consideration, namely that of the transmission of an undulation when the vibrations are in the direction of transmission. We are not only compelled to assume the existence of *undulations*, consisting of vibrations executed in directions perpendicular to the course of the wave, respecting which it is not too much to say, that it is impossible for the mind to conceive the possibility of their existence,—but we are to suppose ourselves* acquainted with an exact law to which

* It is easy to conceive of transversal as the consequence of direct vibration, but I confess myself unable to conceive the possibility of there being a *surface* of transversal vibrations in the same phase: take a sphere for example. At all events, if the hypothesis of transversal vibration is to hold its ground, it must have much more thought bestowed upon it than it has yet received. The most painful circumstance connected with the late history of the undulatory theory, is the manner in which ideas, in themselves perhaps valuable as hints, have been dressed up into a settled theory. A truly philosophical mind, to which the idea of transversal vibrations had once suggested itself, would have set itself to work to discover, if possible, some method by which such motion could be conceived, and would not have rested satisfied so long as a doubt existed as to the perfect feasibility of the scheme. Thus it is that we may account for Dr. Young's not having attempted to carry his first notion any further. He saw, no doubt, the difficulties by which the idea of transversal vibrations was beset; and was well aware that till these were overcome, it was hopeless to attempt to enter into the discussion of their nature and consequences. Fresnel, on the contrary, was satisfied with a series of possibilities, upon which he has built a theory, not only of no value in itself, as having nothing solid to rest upon, but from its crudity and manifold errors, discreditable to himself and to the age by which it has been received.

It may be observed, that in denouncing, as above, undulations consisting of transversal vibrations I had an eye chiefly to their original emission. I still hold it to be impossible that a wave could be emitted consisting of transversal vibrations in the same phase; but at the time of writing the above it had not occurred to me, that although not originally belonging to it, such a character

they are subject. Of the worth of such a theory I leave my readers to judge. The discussion of the remaining portion of it I must defer to another opportunity.

Liverpool, November 8, 1845.

might be impressed on the wave *after* its emission. My views on this subject will be expressed hereafter.

October 3, 1846.

REMARKS OF MR. ARCHIBALD SMITH

RELATING TO PART OF THE LAST PAPER.

(From the *Philosophical Magazine*, Vol. xxviii. p. 48.)*To the Editor of the Philosophical Magazine and Journal.*

GENTLEMEN,—In an article on Fresnel's Theory of Double Refraction, in the Supplement to the December No. of the *Philosophical Magazine*, Mr. Moon has quoted part of an article of mine, in the first volume of the *Cambridge Mathematical Journal*, in which the following passage occurs:—"Let the particle receive a small displacement, the projections of which on the coordinate axes are δx , δy , δz . Then supposing the displacement to be very small, the force of restitution may be taken as proportional to it, so that we have, &c." I am not surprised that Mr. Moon should remark on this passage, "what is meant by the mysterious principle 'supposing the displacement to be very small, the force of restitution may be taken as proportional to it,' I profess myself unable to understand."

The clause in italics, which was added to my manuscript when it was sent to the press, to remove, I believe, what was thought an abruptness in the reasoning, is certainly incorrect when applied to a doubly refracting medium. What was intended to be expressed, no doubt was, that in the case supposed terms involving powers of δx , δy , δz , higher than the first might be neglected. But this expression is only equivalent to the other in the case of a singly refracting medium. I may mention that in the middle of page 7, of the article in the *Cambridge Mathematical Journal*, the word "rays" was, by mistake, substituted for waves. These mistakes are corrected in the second

edition of the first volume of the *Cambridge Mathematical Journal*, which is now printing. As they have been noticed in your Journal, I shall feel much obliged by your inserting this explanation, when you can afford space for it.

Your obedient Servant,

ARCHIBALD SMITH.

25, Old Square, Lincoln's Inn.

Dec. 20.

REMARKS UPON THE FOREGOING.

Mr. Smith is "not surprised" that I should remark, "what is meant by the mysterious principle '*supposing the displacement to be very small the force of restitution may be taken as proportional to it*,' I profess myself unable to understand;" and yet, for anything which appears in Mr. Smith's letter, he might well have been so. He says, "the clause in italics is certainly incorrect when applied to a doubly refracting medium." This remark is itself incorrect. If we have

$$X = \frac{d^2 R}{dx^2} \delta x + \frac{d^2 R}{dx dy} \delta y + \frac{d^2 R}{dx dz} \delta z,$$

and if δr be the radius vector of the particle measured from its point of rest, α, β, γ the inclinations of the radius to the coordinate axes, since

$$\delta x = \delta r \cos \alpha, \quad \delta y = \delta r \cos \beta, \quad \delta z = \delta r \cos \gamma,$$

it is obvious that it is literally true that the force of displacement is proportional to δr , *i.e.* to the displacement itself.

When I animadverted upon the clause in question, I did not consider it at all in the point of view in which Mr. Smith appears to look upon it. It appeared to me that it was meant to be the statement of a distinct physical principle. The principle is incomprehensible enough, it is true, and it is utterly incapable of proof; but Mr. Smith must remember that such objections have, in general, very trifling weight with the followers of Fresnel; and though Mr. Smith may choose to repudiate the

idea of having meant to assume anything of the kind, yet by doing so he renders himself liable to equal animadversion upon another ground. For if Mr. Smith does not mean to assume as a self-evident principle that the force of displacement on any particle depends on the displacement of that particle alone, and is independent of the displacements of the particles surrounding it, he has either committed a gross error in his demonstration, or else he has mistaken the point he had undertaken to prove: *i.e.* either he has mistaken the partial differentials of X, Y, Z for the complete differentials, or else he has considered that he had to prove something short of "the existence of the axes of elasticity." Mr. Smith may have thought with my antagonist, Jesuiticus, that it was sufficient to prove that in a system of particles acting upon each other by their mutual attractions, there are three directions in which if a particle be *suddenly* disturbed, the force of restitution will in the beginning of the motion act in the direction of the displacement; but I must beg to assure him that this is not enough. Mr. Smith has himself described the proposition he had taken in hand to prove as one "on which the whole theory of double refraction depends;" and he must know that the proposition so adverted to is tantamount to nothing short of this, that if x, y, z be the coordinates of any particle measured from the point of rest, the equations which govern its motion will be of the form

$$\frac{d^2x}{dt^2} = -a^2x, \quad \frac{d^2y}{dt^2} = -b^2y, \quad \frac{d^2z}{dt^2} = -c^2z,$$

equations which are not to hold for *an instant, in the BEGINNING OF THE MOTION*, but throughout all time. Does Mr. Smith think the general equation follows from the particular one? Does he think that if the above equations hold "in the beginning of the motion," that they must therefore, as a matter of course, hold throughout the motion? Does he believe this to be a self-evident fact, or one of which we have strong evidence? If he does, I would beg to assure him, on the contrary, that the demonstration of the particular proposition, of itself, affords the most convincing evidence that the general proposition is untrue.

Mr. Smith appears to have a very humble opinion of the judgment of the unfortunate editor, who added the clause to his manuscript: but it appears to me that that individual, whoever he may be, has displayed rather more sagacity in this instance than Mr. Smith himself. What he inserted at any rate bore the semblance of an argument; and though altogether inadmissible, it shewed his consciousness of the defect in the reasoning, which seems entirely to have escaped the notice of Mr. Smith. Indeed that gentleman's blindness appears to have been of so enduring a character, that in taking public notice of a paper in which the futility of his investigation was asserted and proved, he has been insensible to the absurdity of leaving entirely without comment the vital objections to his argument, and confining his efforts to an ill considered correction of a trifling and imaginary error. A certain character in a well known modern novel did not scruple to *chouriner*, who would have scorned to steal. So Mr. Smith is indifferent to the consequences of an error in the higher branches of analysis or physical science, but immediately takes the field in defence of his trigonometry.

Homburg, Sept. 16, 1846.

REMARKS ON A PAPER BY MR. MOON ON FRESNEL'S THEORY OF DOUBLE REFRACTION.

By JESUITICUS.

(From the *Philosophical Magazine*, No. 185.)

THE hypothesis on which Fresnel's Theory of Double Refraction is based is the following:—

“That the displacement of a molecule of the vibrating medium in a crystallized body is resisted by different elastic forces, according to the different directions in which the displacement takes place.”

This is not a mere speculative hypothesis, but is based on experiment. It is found that glass, possessing only the power of single or ordinary refraction, may be made by the application of heat, or by mechanical pressure, to possess that of double refraction.

It is further supposed that the medium is symmetrical with respect to three rectangular axes in space, but in general not symmetrical with respect to any other axis through the same origin. These axes are called the axes of elasticity. It is then proved, that if any particle of the ether be suddenly disturbed, *the other particles remaining quiescent*; the force of restitution developed by such disturbance will not in general be in the direction of the displacement, but only when such displacement is in the direction of the aforesaid axes of elasticity. The elegant demonstration of Smith, quoted by Mr. Moon, is, by Mr. Moon's own shewing, fully adequate to establish the theorem as I have enunciated it, which is doubtless the sense in which Fresnel (the illustrious Fresnel, “whose name is enrolled amongst those which pass not away”) doubtlessly conceived it.

Any one, who understands the subject must at once acknowledge that any theory of light must be, to a considerable extent,

imaginative; and that theory which can explain the greatest number of facts ought to claim the attention of the philosopher more than any other. It is to this that the undulatory theory owes its great celebrity, and of all parts of the undulatory theory that of double refraction is the most extraordinary. It ought to be regarded as a stupendous monument of human ingenuity. It must not be forgotten how admirably the properties of uniaxal crystals follow from the general investigation of the biaxal class; but above all, how from this same investigation conical and cylindrical refraction were discovered by Sir William Hamilton. Such an unexpected refinement as this, which probably would never have been recognised by the mere experimentalist, undirected by the skill of so great an analyst, is surely no slight recommendation of the theory.

Mr. Moon subsequently gives a quotation from Airy's *Tracts*, concerning which he is by no means sparing in arrogant and supercilious criticism. But is it likely that Airy would make such a fool of himself as Mr. Moon earnestly endeavours to represent? It must be remembered that at the time Airy's *Tracts* were published, very little of the undulatory theory was studied or known in Cambridge.

It was the part of this philosopher, therefore, to put everything as much as possible in the clearest and most simple point of view. That there are, and will perhaps long continue to be, difficulties in the undulatory theory, none of its supporters will deny. None of these difficulties are *shirked* or glossed over in the Tract of Airy: he plainly acknowledges each as it arrives. He, no doubt, himself considered the part quoted by Mr. Moon more as an illustration than anything else. Those who wish to see the matter treated with all the generality of which it is capable, are referred to a tract on the Reflection and Refraction of Light at the surface of two contiguous media, by the late famous George Green, in the *Cambridge Philosophical Transactions*.

I have one word more with Mr. Moon. He says that on substituting for u_1 ,

$$u - \frac{du}{dx} h + \frac{d^2u}{dx^2} \frac{h^2}{1.2}, \text{ \&c.,}$$

and for u' ,

$$u + \frac{du}{dx} h + \frac{d^2u}{dx^2} \frac{h^2}{1.2}, \text{ \&c.,}$$

that h is considered small with respect to u .

Does Mr. Moon know anything of analysis? He was eighth wrangler in 1838, and therefore ought to know something. His knowledge, however, has served him miserably on this occasion. The substitutions, stopping at h^2 , merely require that h should in comparison with the *length of a wave*, not with respect to u .

REPLY TO JESUITICUS.

(Altered from the *Philosophical Magazine*, for March, 1846.)

AN anonymous writer, who subscribes himself 'Jesuiticus,' prefaces certain observations upon my first paper on the Theory of Double Refraction, with the following remarks:—

"The hypothesis on which Fresnel's Theory of Double Refraction is founded, is the following: 'That the displacement of a molecule of the vibrating medium in a crystallized body is resisted by different elastic forces, according to the different directions in which the displacement takes place.'"

He then proceeds to make some remarks upon the reasonableness of this hypothesis, which it is not my present purpose to dispute: but I must beg to observe, *en passant*, that as a matter of fact, it is not true that the above is the hypothesis upon which Fresnel's Theory of Double Refraction is based. It rests on a lower level still. The true basis of the theory is the assumption, that the ethereal medium consists of particles separated by finite intervals (to use a well known but somewhat improper mode of expression), acting upon each other by their mutual attractions. From this principle the so-called fundamental hypothesis of Jesuiticus is a sufficiently easy inference. I have thought it necessary however to remark upon this inaccuracy, as, from the extraordinary want of precision of the writers upon this subject, it is somewhat difficult to say what is their real starting-point; at the same time, that in order to make a proper estimate of Fresnel's theory, and of the degree of skill and judgment with which he has worked it out, it is very desirable that that fact should be clearly ascertained.

Jesuiticus goes on to say, "It is then proved that if any particle of the ether be suddenly displaced, the *other particles remaining quiescent*, the force of restitution developed by such disturbance will not, in general, be in the direction of the displacement, but only when such displacement is in the direction

of the aforesaid axes of elasticity. The elegant demonstration of Smith, quoted by Mr. Moon, is, by Mr. Moon's own shewing, fully adequate to establish the theorem as I have enunciated it, which is *doubtless* the sense in which Fresnel (the illustrious Fresnel 'whose name is enrolled amongst those which pass not away') *doubtlessly* conceived it."

Now, as to the fact which Jesuiticus appears to consider so emphatically free from doubt, as to make it necessary for him to reiterate his conviction to that effect almost in the same line, I must confess that I do not altogether acquiesce in his opinion. I confess myself willing to believe that Fresnel, in the course of his long and awkward investigation (which is by no means possessed of that limpid clearness which characterizes Mr. Smith's) had fallen into an error, rather than that he in cool blood proposed the preposterous argument which Jesuiticus would here attribute to him. Be this as it may, however, I admit that Mr. Smith's demonstration is fully adequate to establish the theorem as Jesuiticus has enunciated it; but I must assure Jesuiticus that, unless the demonstration establishes a great deal more than the theorem so enunciated, it is not, for the purpose for which it is adduced, worth the paper it is written upon. What is the use of considering the impossible case of a single particle suddenly disturbed while all the others remain quiescent, and then reasoning upon what takes place in *the beginning of the motion in that case*, as if the same held good *throughout the motion* in the *actual case when all the particles are vibrating together*, when it is perfectly certain that it does not?

Jesuiticus says, "Any one who understands the subject must at once acknowledge that any theory of light must be to a considerable extent imaginative; and that theory which can explain the greatest number of facts ought to claim the attention of the philosopher more than any other."

Of the justice of the remark contained in the first part of the above sentence, Fresnel's Theory of Double Refraction is no doubt a remarkable proof. In the sentiment of the second clause I am disposed to concur, with the reservation however that some portion of the credit due to a theory depends upon

its antecedent probability; for it may happen that a theory may be contrived so fantastical as to require just as much explanation as the phenomena it was intended to elucidate; as is the case with Fresnel's Theory of Secondary Waves emanating from the general front. But mark how our friend Jesuiticus proceeds: "It is to this that the undulatory theory owes its great celebrity, and of all parts of the undulatory theory, that of double refraction is the most extraordinary. It ought to be regarded as a stupendous monument of human ingenuity. It must not be forgotten how admirably the principles of uniaxal crystals follow from the general investigation of the biaxal class; but above all, how from this same investigation, conical and cylindrical refraction were discovered by Sir William Hamilton."

I would now ask Jesuiticus, what is the hypothesis upon which Fresnel professes to explain the separation of the ray? whether it is not substantially what I have stated it to be in the former part of this paper? And if so, I appeal to the world whether I have not shewn incontrovertibly in my two papers* on this subject, contained in the two last numbers (184, 185) of the *Philosophical Magazine*, that Fresnel entirely fails to explain the separation of the ray on that hypothesis. It may be true that some of Fresnel's expressions for the disturbance in polarized waves may contain in them certain elements of truth (though, for my part, I should be sorry to answer for any of them); but they do not on that account afford any evidence of the truth of his principles, for this plain reason, that they do not follow from them. It may happen that from the ruins to which this great theory must soon, if it be not already, reduced, some fragments may be gathered which may form part of a new and more durable edifice;† but

* The second of these is now incorporated in one, which appears in a subsequent part of this work.

† Let those who think that Fresnel's wave surface may be maintained upon experimental grounds, endeavour so to maintain it; and if they succeed in their endeavours, let Fresnel take the credit of the invention. But let it be remembered that *this has not been done yet*; and it may be observed, that whatever fame may be rescued to Fresnel from the wreck of his present great reputation, must always be dimmed by his numerous and enormous errors, which exceed those of any mathematical writer whatever.

Jesuiticus may take my word for it, that, notwithstanding all his vapouring about Fresnel, the time is at hand when that person's theory will be considered as a stupendous monument of anything rather than ingenuity. As to the supposed discoveries of conical and cylindrical refraction, had Jesuiticus been aware of their real nature, he would not have ventured to bring them so prominently forward.

Jesuiticus asks, "Is it likely that Mr. Airy would make such a fool of himself as Mr. Moon earnestly represents?" I never had any intention of attaching to the eminent individual alluded to the very unseemly epithet made use of by Jesuiticus: but, notwithstanding his elegant apostrophe, and notwithstanding the ingenious criticism contained in the latter part of his paper, I am still of opinion that in the investigation quoted and commented on in my first paper on Double Refraction, Mr. Airy *committed the error I there stated him to have committed*, i.e. of having neglected terms without a shadow of reason; and I further reassert, that *the whole investigation*, if meant to apply to the motion of a particle under *bonâ fide* physical circumstances, is *erroneous from beginning to end*. If Jesuiticus has any doubt with regard to the latter point, I shall at any time be happy to relieve him from it. The error of approximation I shall consider presently; but I would first direct attention to another assertion of Jesuiticus, respecting Mr. Airy. He says, "that there are, and perhaps will long continue to be, difficulties in the undulatory theory, none of its supporters will deny. None of these difficulties are *shirked* or glossed over in the Tract of Airy; he plainly acknowledges each as it arrives." Now I apprehend that this statement is calculated to convey a very incorrect idea of the true state of the case. Mr. Airy does indeed, for the most part, direct attention to such of the difficulties connected with the subject as he appears to have been aware of, but this he effects in a very inadequate and partial manner. Thus, in the early and strong part of the theory, a great deal is made of the difficulty arising from the fact of there being nothing in the theory of sound analogous to the different velocities of luminous waves in transparent bodies, where it is in fact rather doubtful whether there

is any difficulty at all; while we have the following very mild *caveat* against perhaps the wildest of all Fresnel's extravagances—the ascribing the change of velocity of a wave in passing from the free ether into a transparent medium “to the particles of ether, while retaining the same attractive force, inside of glass, &c., becoming loaded with some matter which increases their inertia in the ratio $1:n$ without increasing their attraction,” (vide *Tracts*, 2nd edit. p. 354); respecting which Mr. Airy is contented to observe, “Perhaps this supposition is *hardly* reconcilable with that made in the last proposition.” The experience of so humble an individual as myself may possibly have little weight with Jesuiticus; but I can assure him that, whatever he may think of Mr. Airy's fairness, it was the striking difference of tone adopted by that gentleman, in the parts of his work just referred to, which, when I first read the Tract on Optics, as a student at the university, opened my eyes to the suspicious nature of most of the speculations there brought forward.*

Jesuiticus concludes as follows: “I have one word more with Mr. Moon. He says, that in substituting for u_1

$$u - \frac{du}{dx} \cdot h + \frac{d^2u}{dx^2} \frac{h^2}{1.2} + \&c.$$

and for u' ,

$$u + \frac{du}{dx} h + \frac{d^2u}{dx^2} \frac{h^2}{1.2} + \&c.,$$

that h is considered small with respect to u .

“Does Mr. Moon know anything of analysis? He was eighth wrangler in 1838, and therefore he ought to know something. His knowledge however has served him miserably on this occasion. The substitutions stopping at h^2 merely require that h should be small with respect to the *length of a wave*, not with respect to u .”

Now, it is true that if we make the special hypothesis that the disturbance is of the form

$$u = a \cos \frac{2\pi}{\lambda} (vt - x),$$

* Jesuiticus, with characteristic bad taste, makes an allusion to the late Mr. Green, of Caius College, in such terms as, to those unacquainted with the real merits of that respected person, would tend to throw on his pretensions some degree of ridicule.

this difficulty is avoided. But is it quite certain that Mr. Airy can shelter himself under this plea? If that gentleman really considered that his investigation rested upon this particular assumption, how did he happen not to advert to it? His investigation is presented in a general form, the result deduced by it is in a general form, and this result is afterwards made use of in a subsequent part of Mr. Airy's work (*Tracts*, 2nd edit., p. 354). Not a syllable is said of any limiting hypothesis as to the nature of the vibration. If we are to assume

$$u = \cos \frac{2\pi}{\lambda} (vt - x),$$

what is the use of taking the general values of u_1 and u' , deduced by Taylor's theorem? It is true that in a recent edition (1842) Mr. Airy subjoins a note to his demonstration (which he still retains unaltered in his text) in which he takes up the precise ground suggested by Jesuiticus;* but the question is, whether at the time when that demonstration was originally written Mr. Airy was aware of the necessity of making this assumption? If an explanation were required in 1842, after the university had turned out some hundreds of students, more or less fully acquainted with the undulatory theory, how much more so in 1831, at a time when, as Jesuiticus himself suggests, "very little of the undulatory theory was studied or known at Cambridge."—This mode of explanation, by the aid of a particular hypothesis, has, no doubt, some date. Does it occur in Fresnel's original investigation? if so let Mr. Airy, or his champion Jesuiticus, cite evidence of the fact. If it does not, we shall be slow to believe but that Mr. Airy committed the error I stated him to have committed. If it does, it will remain to be considered whether, although at one time aware of it, Mr. Airy had not subsequently lost sight of it, or thought it unnecessary.

I can assure Jesuiticus that there is nothing very incredible in supposing Mr. Airy to have committed such an oversight.

* It is, perhaps, scarcely necessary to mention that when I first commented upon Mr. Airy's investigation, I was not aware of his having adopted this line of justification.

It would not be the first he has made, nor perhaps the greatest. Even his recent explanation betrays a singular confusion of thought with regard to this subject. In the note above referred to, he says, "If h is so large with regard to the length of a wave that the terms after h^2 cannot be safely neglected, we may, by assuming a form for the function expressing u , integrate the equation

$$\frac{d^2u}{dt^2} = - \left(1 - \frac{1}{2^{\frac{3}{2}}} \right) \frac{m}{h^3} (2u - u_1 - u')."$$

How are we to know whether h is "so large with regard to the length of a wave that the terms after h^2 cannot be safely neglected," except by assuming

$$u = A \sin \frac{2\pi}{\lambda} (vt - x)?$$

The fact is, that so far from our knowledge of the relation of h to λ leading us to this assumption, it is the assumption itself which alone suggests the existence of any such relation.

Mr. Airy says, "If h is so large, &c.," as if there could be any doubt of the fact! The letter h here represents the ordinate of any particle measured from that whose motion is being considered, although, from the turn Mr. Airy gives to the investigation, it might appear to mean nothing more than the distance between the rows of particles. Mr. Airy assumes that the motion of the particle is principally affected by the forces exerted upon it by some half-dozen particles immediately surrounding it. Such a supposition is admissible only provided it does not involve any considerations different from what occur when the whole medium is taken into account. But if we take account of the whole medium, h may have any magnitude whatever; hence it is impossible to argue from Mr. Airy's investigation, as originally proposed, to the actual case.

As a further specimen of Mr. Airy's extraordinary views I may observe that, in this same note he endeavours to derive from his formula a proof that different rays may have different refrangibility: in which respect (admitting his data) he to a certain extent succeeds; but it follows from his

result (as no one can know better than himself) that the same circumstance must also obtain in the free ether, which is contrary to the fact; so that at all events *this* is not the true explanation.—I have elsewhere remarked that I am unable to appreciate the support which a theory can derive from an explanation which no one believes; but Mr. Airy seems to entertain a different opinion in this respect.

Lastly, I would observe that Mr. Airy's investigation, let it be bolstered up as it may, is incomplete, as not taking account of the motion, save in one direction. When account is taken of the entire motion, and the effect of the whole medium is also considered, it reduces itself to a particular case of Mr. Kelland's theory, which I have elsewhere shewn to be entirely erroneous.

So much for the justice of my criticism on Mr. Airy—now for the fairness of Jesuiticus's remarks upon me. The case stands thus: Mr. Airy discusses a problem (in one respect at least) in a general manner. The result is in a general form, and this general form is subsequently made use of by Mr. Airy in another part of his work. In examining the investigation I perceive it to be altogether erroneous, but, to save trouble, I point out one particular error which is fatal to the argument as it is actually propounded. But Jesuiticus insists that, although the argument is false in its actual form, it may be supported by means of a subsidiary hypothesis; and as I have not adverted to this circumstance, he thinks it necessary to ask whether I "know anything of analysis?" Such a question from one who has swallowed all the absurdities of Fresnel's theories, occurs to me as somewhat ridiculous; but as Jesuiticus appears to have a rather vague notion of what constitutes proper evidence of ignorance, I will here indulge him with an illustration.

Jesuiticus considers that Fresnel's Theory of Double Refraction "ought to be regarded as a stupendous monument of human ingenuity." He therefore, no doubt, considers that theory to be true. He must, moreover, admit that the principles upon which it is founded are so. He knows very well

that one of those principles is, that the equations of motion of the particle xyz , are

$$\frac{d^2x}{dt^2} = -a^2x, \quad \frac{d^2y}{dt^2} = -b^2y, \quad \frac{d^2z}{dt^2} = -c^2z;$$

and he cannot but be aware that, strange as it may appear, the evidence by which these equations are established implies the fact that the motion of the particle is entirely independent of that of the surrounding medium; or that

$$x = A \cos (at + \alpha), \quad y = B \cos (bt + \beta), \quad z = C \cos (ct + \gamma),$$

where $ABC, \alpha\beta\gamma$, are entirely independent of every circumstance connected with the rest of the medium. When therefore Jesuiticus assumes that the equation

$$u = A_1 \cos \frac{2\pi}{\lambda} (vt - x_1)$$

represents the motion of the same particle, it is evident that he is *ignorant* of the fact that he is making an assumption which is consistent with the principles he has undertaken to defend.

Now what is the ignorance Jesuiticus thinks to have detected in me? Had I adverted to the particular hypothesis when I wrote my criticism on Mr. Airy's investigation, I might still have condemned the latter on the ground I did, as failing to prove what it purported to prove. I knew the investigation to be erroneous; but in choosing my objection to it, I selected that which seemed to present the most novelty, and at the same time exhibited most conspicuously the gross carelessness with which it had been conducted. The only ignorance Jesuiticus can convict me of therefore is, that I did not know it was Mr. Airy's intention to defend his investigation on this ground; a circumstance which, even admitting that Mr. Airy originally had such an intention, would be perfectly compatible with a very considerable knowledge of analysis. Jesuiticus's query therefore was uncalled for, as well as impertinent. If I chose I might retaliate, but I can be merciful even to one who has not been ashamed to indulge in a personal sneer in a publication to which he has not had the courage to put his name. I have shewn Jesuiticus to be guilty of a gross piece of ignorance as to

the consequences of the principles which he himself professes, and I could probably expose him still further in his way; but I do not on that account think it necessary to suggest a doubt as to whether he “knows anything of analysis.” While entertaining my own opinion as to his sagacity, I can still allow him to possess about as much knowledge of analysis as the generality of those who study the undulatory theory at Cambridge.

As to my degree, which Jesuiticus has thought proper to distinguish with so particular a notice, I can only say that I have satisfaction in the enjoyment of my humble honors which it is possible that he does not possess—which is, that *they have not been attained by adopting every error of every author*. It is possible, moreover, that I have some claim on the respect of mathematicians beyond what my degree may confer. If Jesuiticus will condescend to acquaint us with his identity, I shall not be afraid to measure my pretensions with his own.

In conclusion, I would recommend Jesuiticus to leave Mr. Airy to fight his own battles. That gentleman is in a position in which a frank confession of error will avail him more than all the (masters of?) arts of the ‘Society of Jesus.’*

Liverpool, Dec. 9, 1846.

* I do not know why my antagonist styles himself a ‘Jesuit.’ From his practice of begging the question, I should have guessed him a ‘Mendicant.’

A REFERENCE TO FORMER CONTRIBUTIONS

TO THE PHILOSOPHICAL MAGAZINE ON PHYSICAL OPTICS.

By Prof. POTTER, A.M., F.C.P.S., late Fellow of Queens' College,
Cambridge.

(*From the Philosophical Magazine*, No. 186.)

WITHOUT the slightest wish to interfere in the controversies of others, I now beg to refer the readers of the *Philosophical Magazine* to my papers in the Magazines for January 1840, and May 1841. In the former, at page 20, I have shewn Mr. Green's formula for the intensity of reflected light to fail entirely as a representation of nature; and in the latter I have shewn the peculiar refraction near the optic axes of biaxal crystals not to be represented by Sir William Hamilton's analytical deductions from Fresnel's equation to the wave surface in biaxal crystals.

“The anonymous correspondent ‘Jesuiticus’, in the last No., refers to those analytical researches triumphantly in favour of the undulatory theory of light. I do not write to disturb the philosophical opinions of ‘Jesuiticus,’ but to remind the readers of the Magazine where they will find the discussion of the points referred to.”

ON DOUBLE REFRACTION.

(Continued.)

(Altered from the *Philosophical Magazine*, No. 185, with additions.)

IN my last paper I brought under consideration the two preliminary investigations entered into by Fresnel, with a view to facilitating his theory of the separation of the ray in biaxal crystals, viz. the proof of the existence of the axes of elasticity, and that of the law according to which the velocity of a wave consisting of rectilinear transversal vibrations may be inferred from the motion of a single particle: and I shewed in the first place that nothing like axes of elasticity can exist, and in the second, that the proposed deduction of the law of propagation is entirely erroneous. With regard to this latter investigation I might have adopted a different course. The scheme of Fresnel's argument appears to have been, by the aid of the axes of elasticity to ascertain the motion of a single particle, and then, by an independent method, to investigate the effect of that particle on the surrounding medium. Now it so happens that, not only is the particular conclusion at which Fresnel has arrived in this last respect entirely fallacious, as I have shewn, but the existence of the axes of elasticity, upon which the whole of his subsequent superstructure is built, affords the most convincing evidence that *no motion whatever can be propagated from the original particle to the particles around it.*

Thus, if the axes of elasticity exist, we have for the equation of motion of the particle x, y, z ,

$$\frac{d^2x}{dt^2} = -a^2x, \quad \frac{d^2y}{dt^2} = -b^2y, \quad \frac{dz}{dt^2} = -c^2z;$$

whence by integration we get

$$x = A \cos (at + \alpha),$$

$$y = B \cos (bt + \beta),$$

$$z = C \cos (ct + \gamma),$$

where $A, B, C, \alpha, \beta, \gamma$, are constants, which can be determined from a knowledge of the circumstances of the particle's motion at a given time: and as it assumed in the investigation of the axes of elasticity that the motion of the surrounding particles has no effect upon the original particle, it follows that these quantities are entirely independent of every circumstance connected with the surrounding medium. Thus, without some special interposition of Providence directed towards this particular particle, it will never move at all; and, once set in motion, it will vibrate for ever, entirely irrespective of the state of rest or motion of the other particles: as might have been anticipated, as the other particles exercise no influence upon it, conversely it exerts none upon them: or, in a word, *so far from Fresnel's law of propagation being true, no motion whatever will be propagated.*

Now, I would observe, that it would have been sufficient to overthrow Fresnel's theory, had I contented myself with disproving either the existence of the axes of elasticity, or the investigation of the law of propagation; or if I had shewn that the existence of the axes of elasticity negatives the possibility of wave motion: but I have thought proper to expose the fallacy of each step of the argument, in order to make known the character of the man who could propose such a theory, and to expose the more effectually the enormous mass of error which has so long been suffered to pass current. For the same reason I shall enter into a detailed consideration of the remaining part of Fresnel's theory, which I shall here give in the words of Mr. Airy.* (*Vide Airy's Tracts*, p. 345.)

* Sir John Herschel appears to have been so completely mystified in this part of his subject by the extraordinary ratiocination of Fresnel, that he adds an important blunder of his own to the already abounding provision furnished by the latter. According to Sir John Herschel, (*vide Treatise on Light*, Art. 1007,) if a plane wave be incident on a doubly-refracting crystal, having parallel faces, the two emergent waves will be inclined to each other. This may be true, but if so, it must revolutionize the theory of Optics.

“Suppose that in fig. (5) MN is the front of a wave, or by the definitions of (20) and the assumptions of (99) and (100), all the particles in the plane, of which MN is the projection, are moving with equal motions in that plane. In general the force which acts on these particles in consequence of their displacement, is not in the direction of the displacement, and is not even in the plane MN . Resolve it into two; one perpendicular to the plane, and the other parallel to it. The former of these may be neglected, because it can only produce a motion which by (101) is not sensible to the eye. The latter, though in the plane, will not generally be in the direction of the displacement. It is impossible then to find what motions will be caused by this displacement without resolving it. If we can resolve it into two, such that the force produced by each displacement is in the direction of that displacement, then we can find for each of these separately the motions that will result from it. It is clear now that we have fallen on a case exactly similar to that of (104)” [the redoubtable investigation professing to give the law of transmission of transversal vibrations], “and the conclusion is the same, namely, that there will be two series of waves travelling with different velocities.”

The above is meant to apply to the case of uniaxal crystals, but the principle is the same when the crystals are biaxal; though, on account of the more complicated analysis, the application is somewhat different. In this case, if X, Y, Z be the inclinations of the radius vector to the axes of elasticity, $a^2 \cos X, b^2 \cos Y, c^2 \cos Z$ the forces upon the particle, v^2 the whole force parallel to the radius vector, u^2 that perpendicular to it, X_1, Y_1, Z_1 the inclinations of u^2 to the axes, we have

$$\begin{aligned} u^2 \cos X_1 &= a^2 \cos X - v^2 \cos X, \\ &= (a^2 - v^2) \cos X, \\ u^2 \cos Y_1 &= (b^2 - v^2) \cos Y, \\ u^2 \cos Z_1 &= (c^2 - v^2) \cos Z; \end{aligned}$$

and if the equation to the front be

$$lx + my + nz = 0,$$

we must have, when the whole force perpendicular to the radius vector is perpendicular to the front,

$$\left. \begin{aligned} l &= \cos X_1 = \frac{a^2 - b^2}{u^2} \cos X, \\ m &= \cos Y_1 = \frac{b^2 - v^2}{u^2} \cos Y, \\ n &= \cos Z_1 = \frac{c^2 - v^2}{u^2} \cos Z, \end{aligned} \right\} \dots\dots\dots (1);$$

and we have

$$l \cos X + m \cos Y + n \cos Z = 0 :$$

hence, eliminating $\cos X$, $\cos Y$, $\cos Z$ from this last by means of (1), we have

$$\frac{l^2}{v^2 - a^2} + \frac{m^2}{v^2 - b^2} + \frac{n^2}{v^2 - c^2} = 0 \dots\dots\dots (2),$$

an equation which gives two values of v^2 , and therefore two sets of values of X , Y , Z corresponding to the positions of the radius vector, in which the whole force perpendicular to the front acts along that line. It is easily shewn that these directions are at right angles to each other, and correspond with the maximum and minimum values of v^2 , or that they are identical with the greatest and least diameters of the section of the surface of elasticity made by the plane of the front.

It will here be necessary, however, to advert to one or two circumstances which Fresnel and his commentator appear to have overlooked.

It is assumed that the resolved part of the force perpendicular to the front "may be neglected, because it can only produce a motion which is not sensible to the eye." Is this quite clear? Is it quite certain that the force perpendicular to the front has no effect on the motion parallel to the front? Though for certain purposes it may be allowable to neglect the force perpendicular to the front, it must not be forgotten that it still *exists*; the consequence of which is that, even supposing the motion parallel to the front to be rectilinear, the actual motion is not so; so that the true place of the particle at a given time is not to be sought in a line in the front of the wave, but in a plane perpendicular to the front.

Let X_1, Y_1, Z_1 be the inclinations of the true radius vector to the axes of elasticity; x, y, z those of its projection on the front of the wave, which we will suppose to satisfy the equation (2).

$$\text{Let } Lx + My + Nz = 0 \dots\dots\dots (3),$$

be the equation to a plane passing through the radius vector perpendicular to the front; then

$$L \cos X_1 + M \cos Y_1 + N \cos Z_1 = 0 \dots\dots(4),$$

$$Ll + Mm + Nn = 0 \dots\dots(5),$$

$$\text{also } L \cos X + M \cos Y + N \cos Z = 0 \dots\dots(6).$$

The true forces upon the particle, according to the theory of the axes of elasticity, are

$$a^2 \cos X_1, \quad b^2 \cos Y_1, \quad c^2 \cos Z_1;$$

therefore the equations to the direction of their resultant are

$$\frac{x}{a^2 \cos X_1} = \frac{y}{b^2 \cos Y_1}, \quad \frac{x}{a^2 \cos X_1} = \frac{z}{c^2 \cos Z_1};$$

and, in order that this may coincide with the plane whose equation is (3), we must have

$$La^2 \cos X_1 + Mb^2 \cos Y_1 + Nc^2 \cos Z_1 = 0 \dots(7).$$

Eliminating L and M successively between (5) and (6), we get

$$(l \cos Y - m \cos X) M + (l \cos Z - n \cos X) N = 0,$$

$$(l \cos Y - m \cos X) L + (n \cos Y - m \cos Z) N = 0.$$

From these last, and from (4) and (7), we obtain

$$\left. \begin{aligned} (n \cos Y - m \cos X) \cos X_1 + (l \cos Z - n \cos X) \cos Y_1 \\ + (m \cos X - l \cos Y) \cos Z_1 \end{aligned} \right\} = 0,$$

$$\left. \begin{aligned} a^2 (n \cos Y - m \cos X) \cos X_1 + b^2 (l \cos Z - n \cos X) \cos Y_1 \\ + c^2 (m \cos X - l \cos Y) \cos Z_1 \end{aligned} \right\} = 0,$$

$$\therefore (a^2 - b^2) (n \cos Y - m \cos Z) \cos X_1 + (c^2 - b^2) (m \cos X - l \cos Y) \cos Z_1 = 0,$$

$$\left. \begin{aligned} (b^2 - a^2) (l \cos Z - n \cos X) \cos Y_1 \\ + (c^2 - a^2) (m \cos X - l \cos Y) \cos Z_1 \end{aligned} \right\} = 0,$$

and we have

$$\cos^2 X_1 + \cos^2 Y_1 + \cos^2 Z_1 = 1 ;$$

therefore

$$\tan^2 Z_1 = \left(\frac{m \cos X - l \cos Y}{a^2 - b^2} \right)^2 \cdot \left\{ \left(\frac{c^2 - b^2}{n \cos Y - m \cos Z} \right)^2 + \left(\frac{c^2 - a^2}{l \cos Z - n \cos X} \right)^2 \right\} ;$$

from which it follows, that there are only two positions of the radius vector in the plane $Lx + My + Nz = 0$, in which the force upon the particle lies in that plane; so that if at any moment the particle were actually moving in a plane perpendicular to the front, the next it would cease to do so: from which consideration it is plain that the polarization of the rays spoken of by Fresnel is entirely visionary, that his law of propagation is altogether inapplicable; and that from this consideration alone, independently of every other, his whole theory falls to the ground.

Nor is this all. Since it is assumed by Fresnel that we can neglect the force perpendicular to the front in considering the motion in the front, and as the whole force in the front when the particle is disturbed along the greatest diameter is parallel to that line, we may take for the equation of motion parallel to the greatest diameter

$$\frac{d^2u}{dt^2} = Au,$$

where u is the ordinate measured parallel to the diameter, and A is a constant.

Let r be the true radius vector of the particle (supposed in the plane of the front), x, y, z its coordinates measured parallel to the axes of elasticity; then the forces parallel to those axes are a^2x, b^2y, c^2z , and the whole force along the greatest diameter (if α, β, γ be its inclinations to the axes of elasticity) will be

$$a^2x \cos \alpha + b^2y \cos \beta + c^2z \cos \gamma ;$$

and this must coincide with Au where u is the projection of r on the greatest diameter,

$$= x \cos \alpha + y \cos \beta + z \cos \gamma,$$

which is impossible in a doubly refracted crystal: whence it follows that it is not true, as Fresnel supposes, that the effect of the disturbance on the particle is the same as the combined effects of two disturbances communicated in the direction of the greatest and least diameters respectively, and *calculated separately*. Thus, admitting every previous step of the investigation,—admitting the existence of the axes of elasticity, and waiving their incompatibility with wave motion,—admitting the general possibility of transversal vibrations, and the truth of the law according to which they are supposed to be propagated,—admitting the assumption (of which we have not a shadow of evidence, and which, in fact, may be shewn to be untrue) that the force perpendicular to the front may be neglected in considering the motion in the front, and passing over the fact that, whatever may be the case under other hypotheses, under Fresnel's no transversal vibration could take place, it is plain that, *if it did*, it would not be such as Fresnel has supposed.

I have now come to the conclusion of my examination of Fresnel's Theory of Double Refraction as proposed by himself; and for convenience of reference, though at the expense of repetition, I shall recapitulate my objections to it, which are briefly as follows: That there are no axes of elasticity—that if there were there would be no wave motion; that the possibility of undulations consisting of transversal vibrations has never been shewn—much less has the law of propagation, to which Fresnel assumes them to be subject, been proved; that, granting all Fresnel's assumptions, there would be no polarization or resolution of the ray; and lastly, that if the ray were resolved into two, the components would not be what Fresnel makes them out.

With those who are so ignorant of the laws which regulate the exercise of our faculties, as to suppose that processes so conducted could lead to a successful result, or who are so superior to ordinary considerations of evidence as to imagine that an investigation, every step of which is erroneous, may be right in the end, what I have advanced may have little weight: but I am not afraid to appeal to any man who is

accustomed to follow the dictates of his own judgment, whether I have not redeemed my pledge of proving that, "to whomsoever the idea of transversal vibrations may be due, the researches of Fresnel have not advanced us one step beyond it."

I shall hereafter consider some of the improvements upon this theory which have been proposed by Fresnel's followers; but I shall previously direct attention to his Theory of the Reflection and Refraction of Polarized Light.

ON FRESNEL'S THEORY OF POLARIZATION

WHERE THERE IS NO DOUBLE REFRACTION.

HAVING fully exposed the fallacy of Fresnel's Theory of Double Refraction, I now come to the supplementary part of his theory, where, by the aid of divers contrivances, he seeks to obtain formulæ for the intensity of polarized waves after they have undergone reflection and refraction by singly refracting media. I am aware that that individual's credit is by no means principally founded upon his treatment of this part of his subject, and that if his theory of double refraction be overthrown, there are few who will undertake to break a lance in favour of the strange vagaries in which he has here thought proper to indulge. I have deemed it right, however, to review in detail this part of the subject also, and to point out the errors with which it is replete; not from any malicious motive towards those whose credit may thereby be injured, but in order to make known to future ages the extravagant fallacies which it is possible for very sagacious minds, when once the judgment has been surrendered, to regard with complacency; and at the same time to guard the philosophical student against the demoralizing influence which principles and processes like those I am about to consider are calculated to exercise, in perverting the judgment and destroying all sound habits of thought. I shall now therefore, without further preface, give Mr. Airy's account of the way in which, when "light, polarized in the plane of incidence, falls on a refracting surface of glass, &c.," Fresnel finds "the intensity of the reflected and refracted ray." (Vide Airy's *Tracts*, p. 354.)

"Suppose that the particles of ether, retaining the same attractive force, are inside of glass, &c. loaded with some

matter which increases their inertia in the ratio of $1 : n$, without increasing their attraction.

“ The equation of (103) would be changed to this :

$$\frac{d^2u}{dt^2} = \frac{1}{n} \left(1 - \frac{1}{2^{\frac{3}{2}}} \right) \frac{m}{h} \frac{d^2u}{dx^2}.$$

“ If the solution before were $u = \phi(vt - x)$, the solution would now be

$$u = \phi(vt - x\sqrt{n}).$$

“ The velocity of transmission is diminished therefore in the ratio of $\sqrt{n} : 1$. But we have supposed that the velocity is diminished in the ratio of $n : 1$. Consequently $n = \mu^2$.

“ Now suppose that we have a series of equal quantities of the ether in a line, and that a transverse motion is given to the first, which from the constitution described in (103) it has the power of transmitting to the second, &c.

“ When we arrive at the surface of the glass, we must take volumes of the denser ether, whose dimensions are determined in the direction of the transmission of the wave by lengths proportional to the velocity of transmission, and in the other directions by their correspondence with the quantities of ether which puts them in motion. Thus in fig. (6), if $DF = \frac{BD}{\mu}$, the ether in $ABDC$ may be considered as putting $CDFE$ in motion. Put i for the angle of incidence, i' for that of refraction. The proportion of the lengths in the direction of the ray is $\mu : 1$, or $\sin i : \sin i'$. The proportion of their breadths is $\cos i : \cos i'$. The proportion of the densities is $1 : \mu^2$, or $\sin^2 i' : \sin^2 i$. Combining these proportions, the proportion of the masses is

$$\sin i' \cdot \cos i : \sin i \cdot \cos i',$$

“ Now, if an elastic body impinges on an equal elastic body, it loses its own velocity and communicates to the other a velocity equal to its own : this is similar to the action of one mass of the ether in vacuum on the next. Supposing the similarity of action to apply to the different states of ether at the confines of the medium, we must compare this with the motion of two unequal elastic bodies A and B , after the impact of A with the velocity

V on B originally at rest. It is known that A retains the velocity $\frac{A-B}{A+B} V$, and that B receives the velocity $\frac{2A}{A+B} V$. Substituting for A , $\sin i' \cdot \cos i$, and for B $\sin i \cdot \cos i'$, we find for the motion retained by the external ether, $\frac{\sin (i' - i)}{\sin (i' + i)} \times$ its previous displacement; and for that communicated to the internal ether $\frac{2 \sin i' \cdot \cos i}{\sin (i' + i)} \times$ previous motion of external ether: Now, by a succession of numerous impulses of this kind following a given law, a series of waves with any law of displacement may be produced; and every impulse produces parts in the two media having the proportions given above. If then the original displacement be represented by $a \cdot \sin \frac{2\pi}{\lambda} (vt - x)$, that retained by the external ether, and which produces the reflected ray, must be

$$a \cdot \frac{\sin (i' - i)}{\sin (i' + i)} \cdot \sin \frac{2\pi}{\lambda} (vt - x),$$

and that transmitted to the internal ether, and which produces the refracted ray, must be

$$a \cdot \frac{2 \sin i' \cdot \cos i}{\sin (i' + i)} \sin \frac{2\pi}{\lambda} (vt - x)."$$

With regard to the preliminary part of the above investigation, which has for its object the explanation of the change of velocity in passing from one medium to another, I would observe that, being founded on the investigation of the law of propagation commented on in my first paper on double refraction, it must with that investigation fall to the ground. But this is not all I have to say regarding it; and in the first place I would direct attention to the extravagance of supposing "that the particles of ether, retaining the same attractive force, are in the inside of glass, &c., loaded with some matter which increases their inertia in the ratio of $1:n$, without increasing their attraction." Mr. Airy says, indeed, in a note, "Perhaps this supposition is hardly reconcileable with that made in the last propositions;" but surely any objection to the hypothesis

on this ground is trifling, when compared with that which arises from its own intrinsic absurdity. If we had to *invent* light, such fantastical contrivances might be tolerated; but as this is not our object, and we have simply to discover the machinery already devised and put in action for the bringing about that great phenomenon, it would surely be better to confine our efforts to following out the real indications of the phenomena; and if we cannot succeed in connecting them with agents whose mode of operation is understood or can be conceived, at any rate to abstain from wild conjectures as to causes which are neither rendered probable nor even intelligible by analogy, nor necessarily implied by the facts of the case. It is still more advisable to refrain from such violent hypotheses when they are, as in this case, entirely gratuitous; for every object which Fresnel had in view, in making the above assumption, is equally met by supposing that the distance between the rows of particles, or h , is different within the medium from what it is without it.

But besides being extravagant and uncalled for, this hypothesis is *unsuccessful*—it does not after all explain the phenomena it is intended to account for; for, according to this theory, the velocity of transmission depends solely on the nature of the medium, whereas we know very well that in the same medium we have very different velocities.

So much for the preliminary investigation, now for the principal argument. And here one cannot but admire the boldness of the genius which could assimilate the motions of a number of particles situated at considerable intervals from one another (being kept asunder by some inscrutable means) to that of two rough parallel surfaces in contact, acting the one upon the other in a horizontal direction; and which could moreover suppose that the motion so produced would be precisely the same as that of two perfectly elastic bodies. But laying aside this consideration, the investigation fails entirely. Mr. Airy compares the motion of the two masses of the ether within and without the medium respectively “with the motion of two unequal elastic bodies A and B , after the impact of A , with the velocity V on B originally at rest:” but although,

to start with, the ether within the glass might be considered at rest, yet it can only be so momentarily. It can only be so while the vibration of the external particles immediately contiguous to it is represented by $a \sin \frac{2\pi}{\lambda} (vt - x)$, where $\sin \frac{2\pi}{\lambda} (vt - x)$ is extremely small; whereas Fresnel assumes it to hold for all values of that function. The error here committed is curious, as affording an additional example of the extraordinary principles of investigation which that remarkable person was in the habit of adopting. He proves the existence of the axes of elasticity on the supposition of all the particles but one being at rest, and then he takes it for granted, in the face of all evidence to the contrary, that the same holds when all the particles are in motion: he likewise professes to prove a law of propagation, on the supposition that the disturbance is communicated in the direction of the axis of the system, and then assumes that it holds for every direction; and in this last example he proves a formula which may hold for a single instant, but certainly not more, and then assumes it to hold for ever.

The next investigation (vide Airy's *Tracts*, p. 357) has for its object, when "light polarized perpendicular to the plane of incidence falls on a refracting surface, to find the intensity of the reflected and refracted ray.

"We cannot here use the same kind of reasoning as in (128)," (meaning the investigation which we have just considered) "because the motion of displacement (being in the plane of incidence and perpendicular to the path of the ray) is not in the same direction for any two of the three rays. To overcome this difficulty, M. Fresnel has adopted the following hypotheses. First he supposes that the law of *vis viva* holds, that is, that the sum of the products of each mass by the square of its velocity is constant. (This is certainly true if, as in (128), the masses are supposed to act nearly as elastic bodies. And in all cases of mechanical action it is equal to the sum of all the integrals of *force* $\times$ *space through which it has acted*, which is constant in all the cases of undulation that we can strictly examine, and is probably constant in this.) Next he supposes that the resolved parts of the motion, parallel to the refracting surface,

will preserve, after leaving the surface, the same relation which they have there, and which, if they follow the same laws as those of the impact of elastic bodies, would be thus connected: the relative motions" (*qu.* displacements) "before and after impact will be equal in magnitude but opposite in sign. (This is confessed by M. Fresnel to be purely empirical.) Adopting these hypotheses, and considering the masses to be as

$$\sin i' \cos i : \sin i \cos i',$$

and representing the displacements in the incident, refracted and reflected ray, estimated in the direction nearest to that of a body falling perpendicularly from vacuum on the refracting surface, by a, b, c , we have the following equations,

$$\sin i' \cdot \cos i \cdot a^2 = \sin i \cdot \cos i' \cdot b^2 + \sin i' \cdot \cos i \cdot c^2,$$

$$a \cos i = b \cos i' + c \cos i.$$

Eliminating b ,

$$(\sin 2i' + \sin 2i) c^2 - 2 \sin 2i \cdot ac - (\sin 2i' - \sin 2i) a^2 = 0,$$

$$\text{or} \quad (c - a) \{(\sin 2i' + \sin 2i) c + (\sin 2i' - \sin 2i) a\} = 0.$$

"This equation is satisfied by $c = a$: but that would give $b = 0$, and therefore expresses only total reflection, which would require exactly the same mathematical conditions as those that we have used, but would not correspond to the physical circumstances of the problem now before us. The other is the only solution which we want: it gives

$$c = -a \frac{\tan(i' - i)}{\tan(i' + i)},$$

$$\text{and} \quad b = a \frac{\cos i}{\cos i'} \left\{ 1 + \frac{\tan(i' - i)}{\tan(i' + i)} \right\}.$$

"Hence, if the displacement produced by the incident wave is

$$a \sin \frac{2\pi}{\lambda} (vt - x),$$

that produced by the reflected wave is

$$-a \cdot \frac{\tan(i' - i)}{\tan(i' + i)} \cdot \sin \frac{2\pi}{\lambda} (vt - x),$$

and that by the refracted wave is

$$a \cdot \frac{\cos i}{\cos i'} \left\{ 1 + \frac{\tan(i' - i)}{\tan(i' + i)} \right\} \cdot \sin \frac{2\pi}{\lambda} (vt - \mu x)."$$

Thus far Mr. Airy.

From the first of these formulæ it follows that the tangent of the polarizing angle is equal to the refractive index of the medium. This result at first sight seems a remarkable corroboration of the truth of the expression, but there can be little doubt that the formula has been expressly contrived to evolve such a conclusion. Mr. Airy says, "we cannot here use the same kind of reasoning as in the preceding case," but, in truth, there is no difficulty in making the application by resolving the motions parallel and perpendicular to the refracting surface; and the only reason why this has not been done is, that the result so obtained is not satisfactory. Being foiled in this first attempt which, I have not a doubt, Fresnel made, he naturally and very properly had recourse to the general mechanical formulæ, in order to obtain an equation by which to facilitate his future operations; and for this purpose he appears to have selected the equation of *vis viva*, or what, at least, he has thought proper to call such.

The equation he obtains is

$$(1) \quad \sin i' \cdot \cos i \cdot a^2 = \sin i \cos i' \cdot b^2 + \sin i' \cos i \cdot c^2;$$

and he has the further clue that when $i + i' = 90^\circ$, $c = 0$.

Now when this last relation holds, (1) becomes

$$\cos^2 i \cdot a^2 = \sin^2 i \cdot b^2,$$

so that we have

$$\begin{aligned} \cos i \cdot a &= \sin i \cdot b \\ &= \cos i' \cdot b; \end{aligned}$$

but there seems every probability that any formula which involves a and b , will involve c symmetrically with a . Hence there is reason to suppose that this last equation has been deduced from the equation

$$\cos i \cdot a = \cos i' \cdot b - \cos i \cdot c,$$

by putting $c = 0$, as this last will coincide with (1) when $i + i' = 90^\circ$, only on the supposition that $c = 0$. That Fresnel reasoned

every step of his process exactly in the manner here indicated, were perhaps asserting too much, but that he arrived at his conclusion something in this way, I think there cannot be a doubt; for most assuredly there is nothing in the nature of things to lead us to suppose that this second equation expresses a physical truth, so that it can be considered in no other light than as a mere analytical guess. That this second equation should have the good luck to express a proposition which is *intelligible*, although we have every reason to believe it *untrue*, need not be wondered at when we reflect that, besides that which he actually selected, he had three others to choose out of, namely,

$$\cos i \cdot a = \cos i' \cdot b - \cos i \cdot c,$$

$$\sin i' \cdot a = \sin i \cdot b + \sin i' \cdot c,$$

$$\sin i' \cdot a = \sin i \cdot b - \sin i' \cdot c,$$

any one of which would equally have served his purpose. So much then for this apparent coincidence.

That such reasoning as Fresnel has here employed—resting, as Mr. Airy confesses, on an entirely gratuitous assumption—should turn out to be erroneous, need surprise nobody. That it is altogether inconclusive is obvious; that it is erroneous appears from the following consideration. According to the above formulæ the displacements of the incident and reflected rays immediately contiguous to the refracting surface have, except when $\sin \frac{2\pi}{\lambda} (vt - x)$ is very small, the same sign; or

the incident and reflected vibrations immediately contiguous to the same point in the surface are both in the same direction, *i.e.* both moving towards, or both moving from the refracting body, which, as a general fact, may be fairly asserted to be *impossible*.

As to Mr. Airy's somewhat tardy display of discrimination in speaking of one of the above hypotheses as being purely empirical, it is perhaps even more injurious than his usual want of that quality, as leading the student to believe that the same character of gratuitous assumption does not appertain to other parts of this and the preceding investigation; for example, the manner of calculating the *vis viva*, and the assumption as to the

mode of action in the preceding problem, which are as purely the offspring of the fancy of their author as any other portion of his ingenious theories.

The next investigation undertaken by Mr. Airy has for its object, when "light is evident on the internal surface of glass at an angle equal to or greater than that of total reflection, to find the intensity of the reflected ray."

"The expressions in (128) and (129)" [the preceding investigations] "become impossible. Yet there is a reflected ray, whatever be the nature of the vibrations of the incident light. And on the principle of *vis viva* the intensity of the reflected ray ought to be equal to that of the incident ray, since there is no refracted ray to consume a part of the *vis viva*. And indeed in the last state of the expressions of (128) and (129) before becoming impossible, *i.e.* when $i' = 90^\circ$, each of them becomes = 1. After this the expression for the coefficient of vibrations perpendicular to the plane of incidence, putting $\mu \sin i$ for $\sin i'$, and $\sqrt{(-1)} \sqrt{(\mu^2 \sin^2 i - 1)}$ for $\cos i'$, becomes

$$\frac{\mu \sin i \cos i - \sin i \sqrt{(-1)} \sqrt{(\mu^2 \sin^2 i - 1)}}{\mu \sin i \cos i + \sin i \sqrt{(-1)} \sqrt{(\mu^2 \sin^2 i - 1)}} \dots\dots\dots (a),$$

or $\cos 2\theta - \sqrt{(-1)} \sin 2\theta,$

where $\tan \theta = \frac{\sqrt{(\mu^2 \sin^2 i - 1)}}{\mu \cos i};$

and that for the coefficient of vibrations parallel to the plane of incidence, becomes

$$\frac{\sin i \cos i - \mu \sin i \sqrt{(-1)} \sqrt{(\mu^2 \sin^2 i - 1)}}{\sin i \cos i + \mu \sin i \sqrt{(-1)} \sqrt{(\mu^2 \sin^2 i - 1)}} \dots\dots\dots (a),$$

or $\cos 2\phi - \sqrt{(-1)} \sin 2\phi,$

where $\tan \phi = \frac{\mu \sqrt{(\mu^2 \sin^2 i - 1)}}{\cos i} .,$

"It is improbable that these formulæ are entirely without meaning: what can their meaning be?

"M. Fresnel seems to have considered that, as the direction of the reflected ray and the nature and intensity of the vibration were already established, there remained but one element which could be affected, namely the phase of vibration. And

it seems not improbable that this may be affected, inasmuch as the incident vibration, though it cannot cause a refracted ray must necessarily cause an agitation among the particles of the ether outside the glass. It would seem to us most likely that the ray would be retarded (though the phenomena to be hereafter described compel us to admit that it is accelerated), and in all probability differently according to the direction in which the vibrations take place. Nothing then seems more likely than that 2θ and 2ϕ should express these accelerations: and as they are angles, they must be combined with angles in the expression for the vibration. Thus, for instance, if

$$a \cdot \sin \frac{2\pi}{\lambda} (vt - x)$$

were the expressions for the vibrations perpendicular to the plane of incidence on the supposition that they were not accelerated,

$$a \sin \left\{ \frac{2\pi}{\lambda} (vt - x) + 2\theta \right\}$$

would be the expression on the supposition that they were accelerated."

I forbear giving Mr. Airy's account of the reasoning by which Fresnel attempts to establish that the occurrence of the impossible quantity in the above expressions indicates a change of 90° , as it is not my purpose to comment upon it. Mr. Airy is, no doubt, consistent in believing it to be "improbable" that the above formulæ "are entirely without meaning," as he seems to have no particular horror of the way in which they have been derived, and even appears to have imbibed the notion "that they are fully born out by experiment"; but to any one else it would most likely appear that that has happened which might have been anticipated, namely, that the formulæ have at last brought their inventor into an awkward dilemma. A meaning they indeed bear, and that a very obvious one, which is, that whatever else they may apply to, they do not apply to the internal reflection of light. It is all very well for Fresnel and Mr. Airy to talk of the phase being affected, but what will the latter say to the refracted portion of the ray?

The general formula for the refracted ray, when the light is polarized in the plane of incidence, is

$$a \cdot \frac{2 \sin i' \cdot \cos i}{\sin (i' + i)},$$

and substituting $\mu \sin i$ for $\sin i'$, and $\sqrt{(-1)} \sqrt{(\mu^2 \sin^2 i - 1)}$ for $\cos i'$, this becomes

$$a \cdot \frac{2\mu \sin i \cdot \cos i}{\mu \sin i \cos i + \sin i \sqrt{(-1)} \sqrt{(\mu^2 \sin^2 i - 1)}};$$

or $2a \cos i \{ \cos \theta - \sqrt{(-1)} \sin \theta \},$

where $\tan \theta = \frac{\sqrt{(\mu^2 \sin^2 i - 1)}}{\mu \cos i}.$

If the first formula shews that a ray is reflected, surely this must shew that one is refracted.

It is possible that Mr. Airy is by this time prepared to admit that the investigations of the intensities do not afford a shadow of evidence in favour of the formulæ deduced, and that these last rest solely upon the basis of experiment: but if he contends that we are authorized upon experimental grounds to consider the formulæ for the intensity of the reflected light as so indisputably true that they must bear a meaning under all circumstances, will he argue that the formulæ for the refracted light are of less binding character? and if he is not prepared to do so, will he condescend to explain upon what principle we are to consider the occurrence of the same fact, in the one case an indication that there will be no vibration at all, and in the other that there will be two vibrations succeeding one another at an interval of 90° ?

Mr. Airy tells us (Tract on *Light*, Art. 126) that the three investigations which we have just been considering "cannot be considered as wholly satisfactory;" but he consoles himself with the idea "that they are fully supported by experiment." Who will vouch for this? Will Sir David Brewster, or Professor Potter, or any other anti-undulatist? As to Mr. Airy himself, while he possesses so little candour, or so little discrimination, as to pass over without notice such a remarkable discrepancy between facts and formulæ as that I have just pointed

out in the theory of internal reflexion, we must be permitted to entertain a doubt respecting his competency as an authority.

To an author like Fresnel no subject can present any difficulty. His arguments are spiritual essences, against which the weapons of the vulgar world are unavailing: amidst the tempest of objections they bear a charmed life. Listen to Fresnel, and we have a theory of light perfect in all its parts, so far as investigated, and equal to any emergency. But there is a trifling drawback on the delights of this glittering spectacle—they are visionary. The delightful region we are traversing is a land of dreams, in which the principles which regulate human reason are strangely jostled. We may if we choose for a time amuse ourselves with the illusion, give the reins to the imagination, shut out the necessities of real life—but there must sooner or later be an end of all this; and when a returning consciousness of the actual world, with its stern realities, breaks in upon us, we shall find ourselves, with minds enervated and judgment obscured, left to cope with difficulties which under happier auspices we shrank from as insurmountable.

Liverpool, Oct. 23, 1846.

ON MR. KELLAND'S "THEORY" OF HEAT.

I now propose to examine some of the more prominent suggestions which have been thrown out by the followers of Fresnel, with a view to obviate the glaring defects of his theory. I am induced to this step, not from any particular cogency in the arguments of these gentlemen which I have been able to discover, but from the knowledge that others have been led to place in them a degree of faith greater than mine, to the extent in fact of believing that many points left by Fresnel in an unsatisfactory state have through their efforts been established on a solid basis. As I believe that whatever objections may be urged against Fresnel's processes are equally applicable to those of his followers, that if the first are erroneous the latter are equally so, it appears to me desirable to destroy this last refuge also of a falling theory; a measure by so much the more advisable, as in the execution of it I shall have an opportunity of displaying the reality of the evil ascribed to the reception of Fresnel's theory "in perverting the judgment and destroying all sound habits of thought."

In the following paper I intend to consider Mr. Kelland's method of treating the theory of finite intervals, as developed in his Treatise on the Theory of Heat, and shall endeavour to give an account of his views as far as possible in his own words.

"The particles of matter in all bodies" (*Theory of Heat*, p. 145) "are supposed to be surrounded by particles of a subtle fluid, pervading space and termed caloric, the atoms of which are so much smaller than those of matter, that each material particle may be conceived to be surrounded by a large number of them.

"It is supposed that the particles of caloric repel each other, but attract those of matter, whilst the particles of matter also

repel each other, the function of the distance being always the same, which we shall give reasons for assuming to be that of the inverse square.

"Let xyz be the coordinates of any particle P of caloric when at rest, $x + \delta x, y + \delta y, z + \delta z$ those of another particle Q , $x + \Delta x, y + \Delta y, z + \Delta z$ those of a material particle M , $PQ = r$, $PM = R$, $r\phi(r)$ the law of attraction: suppose the attraction at distance unity of a particle of caloric to be P , and that of a particle of matter M .

"Let $x + \alpha, y + \beta, z + \gamma$, be the coordinates of P at the end of the time γ ,

$x + \alpha + \delta x + \delta \alpha, y + \beta + \delta y + \delta \beta, z + \gamma + \delta z + \delta \gamma$ those of Q ,
 $x + \alpha + \Delta x + \Delta \alpha, y + \beta + \Delta y + \Delta \beta, z + \gamma + \Delta z + \Delta \gamma$ those of M ,
 $r + \rho$ the distance PQ ,
 $R + H$ the distance PM ."

Then

$$(1) \frac{d^2 \alpha}{dt^2} = -P\sigma (\delta x + \delta \alpha) \phi(r + \rho) + M\Sigma (\Delta x + \Delta \alpha) \phi(R + H).$$

"To simplify this equation," Mr. Kelland supposes "that the particles of the medium are arranged in symmetry about the axes," and he also assumes the particles to have a vibratory motion about the axis of x , represented by the equations

$$\alpha = a \cdot \cos(nt - kx),$$

$$\beta = b \cdot \cos(nt - kx),$$

$$\gamma = c \cdot \cos(nt - kx);$$

and further, that the particles of matter have a similar motion represented by

$$\alpha' = A \cos(Nt - Kx + B),$$

&c.

where α', β', γ' denote the displacements of the material particle M .

These last assumptions he subsequently modifies. To shew their consistency with the equation (1) is the principal scope of Mr. Kelland's analysis.

We have $\phi(r + \rho) = \phi(r) + \phi'(r)\rho$, nearly, *provided* ρ and

therefore δa , $\delta \beta$, $\delta \gamma$, are small, and on the same hypothesis it is easily seen that

$$\rho = \frac{1}{r} (\delta x \delta a + \delta y \delta \beta + \delta z \delta \gamma),$$

$$\{\text{since } (r + \rho)^2 = (\delta x + \delta a)^2 + (\delta y + \delta \beta)^2 + (\delta z + \delta \gamma)^2\}.$$

Hence, if the medium be symmetrical,

$$\sigma (\delta x + \delta a) \phi (r + \rho) = \sigma \left\{ \phi (r) + \frac{\phi' (r)}{r} \delta x^2 \right\} \delta a.$$

But it will be found that

$$\delta a = -2a \sin^2 \frac{k \delta x}{2} + \frac{da}{dx} \frac{k \delta x}{k},$$

and, observing that the second term of this expression vanishes under the symbol σ , we have

$$\sigma (\delta x + \delta a) \phi (r + \rho) = -2a\sigma \left(\phi r + \frac{\phi' r}{r} \delta x^2 \right) \sin^2 \frac{k \delta x}{2}.$$

Similarly for the second term in the value of $\frac{d^2 a}{dt^2}$: hence the equation of motion becomes

$$\begin{aligned} (2) \quad \frac{d^2 a}{dt^2} &= 2Pa\sigma \left(\phi r + \frac{\phi' r}{r} \delta x^2 \right) \sin^2 \frac{k \delta x}{2} \\ &- Ma\Sigma \left(\phi R + \frac{\phi' R}{R} \Delta x^2 \right) + Ma'\Sigma \left(\phi R + \frac{\phi' R}{R} \Delta x^2 \right) \cos K\Delta x. \end{aligned}$$

Similarly,

$$\begin{aligned} (3) \quad \frac{d^2 \beta}{dt^2} &= 2P\beta\sigma \left(\phi r + \frac{\phi' r}{r} \delta y \right) \sin^2 \frac{k \delta x}{2} \\ &- M\beta\Sigma \left(\phi R + \frac{\phi' R}{R} \Delta y^2 \right) + M\beta'\Sigma \left(\phi R + \frac{\phi' R}{R} \Delta y^2 \right) \cos K\Delta x. \end{aligned}$$

By substituting the values above assumed for $a\beta\gamma$, $a'\beta'\gamma'$, respectively in these last equations, we obtain

$$(4) \quad - \left(n^2 + 2\sigma f \sin^2 \frac{k \delta x}{2} - \Sigma F \right) a = \Sigma F \cos K\Delta x . a',$$

$$(5) \quad \left(N^2 + 2\Sigma F \sin^2 \frac{K\Delta x}{2} - \sigma f \right) a' = \sigma f \cos k\delta x . a;$$

where

$$P \left(\phi r + \frac{\phi' r}{r} \delta x^2 \right) = f,$$

$$M \left(\phi R + \frac{\phi' R}{R} \Delta x^2 \right) = F.$$

Mr. Kelland here makes a "considerable digression in order to determine the form of the function ϕ ." It is a good maxim 'never to meet one's troubles half way.' This Mr. Kelland seems not to have heard, or to have forgotten, otherwise in the midst of his other difficulties he would not have gone out of his way to prove what every one would be ready enough to concede, viz. that the law of force will be that of the inverse square. But let us see what additional probability Mr. Kelland throws on this assumption.

"Suppose the motion to take place in a vacuum, for which we obtain the following equations,

$$\frac{d^2 a}{dt^2} = 2P a \sigma \left(\phi r + \frac{\phi' r}{r} \delta x^2 \right) \sin^2 \frac{k \delta x}{2},$$

$$\frac{d^2 \beta}{dt^2} = 2P \beta \sigma \left(\phi r + \frac{\phi' r}{r} \delta y^2 \right) \sin^2 \frac{k \delta x}{2};$$

hence therefore, $\beta = b \cos (nt - kx)$,

$$n^2 = -2P \sigma \left(\phi r + \frac{\phi' r}{r} \delta y^2 \right) \sin^2 \frac{k \delta x}{2}.$$

"Now it must be observed that the mode in which this result is produced is one which takes account of the *differences* of the disturbances on the two sides of the particle under consideration. The magnitude of the expression will therefore clearly depend greatly on the portion which arises from the particles next it. In other words, $\phi(r)$ must be some inverse function of r , such that it diminishes as r increases."

I would here observe that it does not follow that, because "the mode in which" the above "result is produced is one which takes account of the *differences* of the disturbances on the two sides of the particle," "the magnitude of the expression will depend greatly on that of the portion which arises from the particles next it;" for, notwithstanding that such were the case, if the force happened to vary as some direct power

of the distance—and we are as yet supposed to know no reason why it should not—it would follow that “the magnitude of the expression” would *not* “depend greatly on that of the portion which arises from the particles next it.” So far therefore from this pretended argument affording us any ground for believing the force to vary as an inverse power of the distance, it actually assumes the point at issue. But to proceed.

Assuming $\phi(r)$ to be an inverse power of r , Mr. Kelland considers it to follow that, “as far as this factor” (meaning I presume the factor $\sigma \left(\phi r + \frac{\phi' r}{r} \delta x^2 \right)$ “is concerned, the particles near P will have the principal influence; but within a certain limit this will not be the case with the factor $\sin^2 \frac{k\delta x}{2}$, which will increase at first until it becomes unity, and then again diminish as δx is increased. The result is obviously this, that the principal part of the term will be that which lies within the value of $\delta x = \frac{\pi}{k}$.”

Upon this “result” Mr. Kelland builds his argument for believing the law of force to be that of the inverse square. Its “obvious” character I must be permitted to doubt. The result may be true, but most certainly it is not obviously so. As δx increases from $\frac{\pi}{k}$ to ∞ , successive increments are given to the value of n^2 , which, “so far as *this* factor is concerned,” have all the same sign. Who will assure us that the aggregate of these last does not exceed the value of that portion which lies within the value $\delta x = \frac{\pi}{k}$? Is there any strong reason to believe that such is not the case? Is there anything “obvious” about the matter either one way or other? And yet the writer who is too fastidious to be content with the evidence afforded by analogy and the law of equal diffusion, is satisfied with a mere *guess* at the solution of this dark mystery.

Returning however to his formulæ, it occurs to Mr. Kelland that his law of force, although it “appears to satisfy the requisite conditions,” fails to account for one circumstance, the

explanation of which appears indispensable to the theory, viz. that in a vacuum there shall be an absolute freedom from variation of velocity due to a variation in the length of the wave."

His method of getting over "this difficulty" is sufficiently characteristic. He conceives that it "arises from the imperfection of our acquaintance with the mode in which the particles, may be arranged."

In this respect I am compelled to differ from Mr. Kelland. As a matter of fact, we have no *knowledge* of the existence of a medium consisting of particles situate at finite* intervals from one another, much less of the nature of its arrangement, yet this circumstance has been no obstacle to Mr. Kelland's *suggesting* such a medium, and even devising a law of symmetry to which he supposes it to be subject. In the same way, though we have no *knowledge* of the mode in which the particles are arranged (nor in fact are likely to have), yet it was perfectly open to Mr. Kelland to *suggest* any arrangement which would suit his purpose. It is the impossibility of *conceiving* any such which constitutes the real "difficulty." No ignorance of a matter of fact need disturb a disciple of Fresnel. According to that school of logic, if one cannot swear that a system of explanation is untrue it is sufficient. But Mr. Kelland here felt it impossible to make a single step in advance. The condition to be satisfied is that

$$\frac{n^2}{k^2} \text{ or } \sigma \left\{ \phi r + \frac{\phi'(r)}{r} \delta y^2 \right\} \sin^2 \frac{k \delta x}{2} \div k^2$$

is independent of k . As far as the first half wave we have

$$\frac{\sin k \delta x}{2} = \frac{k \delta x}{2} - \frac{1}{1.2.3} \left(\frac{k \delta x}{2} \right)^2 + \&c.,$$

as far as the second,

$$\frac{\sin k \delta x}{2} = \pi - \frac{k \delta x}{2} - \frac{1}{1.2.3} \left(\pi - \frac{k \delta x}{2} \right)^3 + \&c.;$$

whence it is evident that

$$\sigma \left\{ \phi(r) + \frac{\phi'(r)}{r} \delta y^2 \right\} \sin^2 \frac{k \delta x}{2} = A + Bk + Ck^2 + Dk^3 + Ek^4 + \&c.$$

* I adopt this expression for its convenience, though sensible of its impropriety.

Who is skilful enough to devise a medium which will be so obliging as to make C finite while $A, B - D, E$, &c. all vanish? And until direct proof to that effect has been given, who will believe that any such can exist?

Nothing daunted by improbabilities, however, Mr. Kelland without hesitation assumes that such an arrangement actually obtains; but surmising that his inability to guess at its nature might attach a stigma to his system, as being unable to surmount this "difficulty," he seeks to overcome it by having recourse to a subsidiary principle, which he considers to be necessarily implied by his original hypothesis. "One thing," says he, "is certain, that when they" (meaning the particles) "are very close together, they act as a mass of particles in contact."—What a satisfaction to meet with 'one thing certain,' one firm rock amidst the ocean of doubt on which we are tossed. While examining Mr. Kelland's theory for the first time, and endeavouring to ascertain the bearing of its several parts amidst the confusion of principles and the errors of analysis, I more than once reverted to this 'one thing certain,' all in fact that I met with which laid claim to that property, overlooking the circumstance, in my joy at the discovery, that this claim rested solely on the credit of our author. But when in a calmer moment I began to look more narrowly to the nature of the proposition on whose certainty I had congratulated myself, I soon found that I had been somewhat too hasty in my felicitations. "When very close together they act as a mass of particles in contact." What was meant by a mass of particles in contact? By some people a rigid body would be considered as such, but this evidently was not intended. Neither could a mass of particles like sand, kept asunder solely by the mutual resistances of their surfaces, though this also seemed to possess some of the qualities of the article prescribed. Reading on however, I became more enlightened.

"Taking it for granted that such is the case" (*i.e.* the above certainty) "we may remove the difficulty thus:

"Let D be the density of the particles in vacuo, then the equation of motion, parallel to y , becomes

$$\frac{d^2\beta}{dt^2} = -k^2n^2D.\beta."$$

Now though I did not pretend to understand (as I do not now) why, when "they act as a mass of particles in contact," this last equation should hold, the introduction of the consideration of density seemed to imply that something like the medium which is the instrument of sound was intended. If this be the case, however, I must say Mr. Kelland has adopted a singular mode of expressing himself. Does he consider that the medium which produces sound consists of a mass of particles in contact? Such a mode of regarding the construction of the atmosphere is certainly somewhat novel, so much so indeed that there is some difficulty in believing that this is exactly what Mr. Kelland meant to express. It is possible that, as in the problem of sound we consider the sonorous medium as made up of immediately contiguous laminæ, he considered this amounted to the same thing as the ultimate particles themselves being immediately contiguous; thus confounding two things very distinct, the ultimate particles which constitute matter, and the imaginary elements into which we may suppose it divided.

But, waiving the objection to the phraseology adopted by Mr. Kelland, and assuming that he meant *bonâ fide* to compare his own to the atmospheric medium, I would observe that it is by no means *certain* that a medium such as he has supposed, consisting of particles placed at finite intervals from each other and mutually repellent, would be endowed with that elasticity which is the characteristic of the medium of sound. It may be true, but it is anything but certain. We know, or at any rate Mr. Kelland is not the person to dispute the fact, that the atmosphere is not a system of homogeneous particles, but that it consists of material particles and a fluid 'termed caloric;' and recent experiments have made it evident that it is to the presence of this last that the elasticity is due. This fact may well beget a doubt as to whether any single fluid consisting of homogeneous particles can be subject to the same laws as those which govern the motions of the atmosphere. It may be said that Mr. Kelland here takes for granted no more than what has been conceded to every writer on the undulatory theory; but I would observe that previous writers have for the most part taken for their postulate nothing

further than this, that the medium by which light is produced is subject to the same laws as those which govern the sonorous medium, while in the present instance Mr. Kelland wishes us to consider as certain not merely that the *real* medium is like the medium of sound, but that *his* medium is so; thus implying, beyond the ordinary hypothesis which is the basis of the undulatory theory, this additional fact, that his is the true medium, which, it must be permitted to add, is so far from being certain that all the evidence yet adduced militates against it.

I have said that Mr. Kelland considers the supplemental principle to which he has here had recourse to be necessarily implied by, or at any rate to be consistent with, his original hypothesis; but so far is this from being the case, the latter is contradicted by the former in a material point. This will appear from the consideration, that he has not only supposed his medium to consist of mutually repelling particles, but likewise *that the motions of the particles are small compared with their actual distances*; whereas, in a medium consisting "of particles in contact" the motions are exceedingly great compared with their distances *inter se*. It is true that this is only of importance as regards the particles in the immediate neighbourhood of that whose motion is being considered; but, as the force varies inversely as the square of the distance, these last are precisely those whose effect is most important. Hence on this ground also it is not 'certain' that Mr. Kelland's medium will act as a mass of particles in contact.

Lastly, with respect to the only undulations of the sonorous medium with which we are acquainted, the vibration is in the direction of transmission, while it is all along Mr. Kelland's object to shew that the vibrations are transversal. Hence, if the fact were true which Mr. Kelland states as certain, it would be a fatal blow to the theory.

By a novel and ingenious combination of the principle of the last equation with that of (3), Mr. Kelland deduces a value for the velocity in a transparent medium,

$$(6) \quad v^2 = \frac{D}{D} V^2 + 2(Q - M) \Sigma F. \frac{\sin^2 k \Delta x}{k^2},$$

where D, D' are the densities in vacuo and within the body respectively, V the velocity in vacuo, Q the repulsion at distance unity of a mass of particles which would occupy the space of a material particle: a conclusion which is so far satisfactory that it gives different values of v corresponding to different values of k , or the length of the wave; it also exhibits a uniform velocity in vacuo. This last triumph however has a considerable drawback attendant upon it, for, according to Mr. Kelland, *in a vacuum there can be no variation in the length of a wave.* From the equations

$$\frac{d^2\beta}{dt^2} = -k^2 n^2 D \cdot \beta,$$

$$\beta = b \cdot \cos (nt - kx),$$

it is evident that

$$-n^2 = k^2 n^2 \cdot D,$$

or

$$1 = k^2 \cdot D.$$

But

$$\lambda = \frac{2\pi}{k}, \quad \therefore \lambda^2 = 4\pi^2 D,$$

a conclusion equally novel and satisfactory.

It does not appear that Mr. Kelland has troubled himself to compare the above value for the velocity with that which results from his original equation. It will be found however that the two are altogether irreconcilable. This will appear as follows.

Since the medium is one of symmetry, we have

$$\Sigma F = \Sigma \cdot \frac{\Delta x^2 + \Delta y^2 + \Delta z^2 - 3\Delta x^2}{R^5} = 0,$$

and similarly $\sigma f = 0$. Hence the equations (4) and (5) become

$$\begin{aligned} -\left(n^2 + 2\sigma f \sin^2 \frac{k\delta x}{2}\right) a &= -2\Sigma F \sin^2 \frac{K\Delta x}{2} a', \\ -\left(N^2 + 2\Sigma F \sin^2 \frac{K\Delta x}{2}\right) a' &= -2\sigma f \sin^2 \frac{k\delta x}{2} a. \end{aligned}$$

Mr. Kelland assumes that $K = k$, $N = n$, which he says is "evident." Now this may be a very allowable hypothesis, but it is certainly not a necessary one; as in fact the phenomena of double refraction would lead us to suppose it pos-

sible that the vibrations of the particles of ether and of the material particles are propagated with different velocities. Assuming the above, however, since by adding the two last equations we obtain

$$(7) \quad N^2 a' + n^2 a = 0,$$

it is evident that $a = -a'$, and equation (3) becomes

$$\frac{d^2 \beta}{dt^2} = 2P\beta\sigma f \sin^2 \frac{k\delta x}{2} + 2M\beta\Sigma F \sin^2 \frac{K\Delta x}{2},$$

$$\text{which gives } v^2 = \frac{n^2}{k^2} = \frac{-2P\sigma f \sin^2 \frac{k\delta x}{2} - 2M\Sigma F \sin^2 \frac{K\Delta x}{2}}{k^2}.$$

This will agree with (6), provided that

$$\frac{D'}{D} V^2 + 2Q \cdot \frac{\Sigma F \cdot \sin^2 \frac{k\Delta x}{2}}{k^2} = -2P \cdot \frac{\sigma f \sin^2 \frac{k\delta x}{2}}{k^2}.$$

But the second member of this equation represents the square of the velocity in vacuo = V^2 ,

$$\therefore \left(\frac{D'}{D} - 1 \right) V^2 + 2Q \cdot \frac{\Sigma F \cdot \sin^2 \frac{k\Delta x}{2}}{k^2} = 0,$$

$$\text{and } \frac{\Sigma F \cdot \sin^2 \frac{k\Delta x}{2}}{k^2} = \text{a constant},$$

which is impossible, or at any rate untrue.

From the above we see pretty clearly the fallacy of Mr. Kelland's explanation of "the variation of velocities in transparent media." The fallacy of the entire theory may be surmised from the equation (7): for from that equation it is evident that a and a' have always different signs. But a and a' represent the motions of any particles taken at random, the one from the caloric, the other from the material substance of the body: whence it appears that all the particles of the caloric at any given time must be moving in the contrary direction to some one material particle, in other words all the particles of caloric at any given time must be moving in the same direction; a circumstance which precludes the possibility of wave motion.

But no longer to keep my readers in suspense as to the fate of this theory, I shall hasten to the catastrophe.

"To find the value of n^2 and N^2 .

"The equations (2) and (3) are now reduced to

$$\begin{aligned}\frac{d^2a}{dt^2} &= 2Pa\sigma \left(\frac{1}{r^3} - \frac{3\delta x^2}{r^5} \right) \sin^2 \frac{k\delta x}{2}, \\ &+ 2Ma\Sigma \left(\frac{1}{R^3} - \frac{3\Delta x^2}{R^5} \right) \sin^2 \frac{k\Delta x}{2} \\ &= T_x a + U_x a \text{ suppose,} \\ \frac{d^2\beta}{dt^2} &= 2P\beta\sigma \left(\frac{1}{r^3} - \frac{3\delta y^2}{r^5} \right) \sin^2 \frac{k\delta x}{2}, \\ &+ 2M\beta\Sigma \left(\frac{1}{R^3} - \frac{3\Delta y^2}{R^5} \right) \sin^2 \frac{k\Delta x}{2} \\ &= T_y \beta + U_y \beta.\end{aligned}$$

"But by addition,

$$T_x + T_y + T_z = 0, \quad U_x + U_y + U_z = 0.$$

"Also it is clear that $T_y = T_z$, $U_y = U_z$;

$$\therefore T_x = -2T_z, \quad U_x = -2U_z;$$

or if

$$\begin{aligned}\frac{d^2a}{dt^2} &= 2n^2a, \\ \frac{d^2\beta}{dt^2} &= -n^2\beta, \quad \frac{d^2\gamma}{dt^2} = -n^2\gamma.\end{aligned}$$

"The solutions of these equations are

$$\begin{aligned}a &= f\epsilon^{n\sqrt{2},t} + g\epsilon^{-n\sqrt{2},t} \\ \beta &= b \cos(nt - kx), \\ &\&c.\end{aligned}$$

"But we assumed the solution of the first equation to be

$$a = a \cos(nt - kx).$$

"It is necessary therefore to alter that assumption in such a manner as still to satisfy the values of δa , Δa , found in (157).

$$(8) \text{ " Let } a = a (\epsilon^{n\sqrt{2},t} - \epsilon^{-n\sqrt{2},t}) \cos(kx + c) = a' \cos(kx + c),$$

then δa will, by the same operation as in (157), give the same results; and

$$\begin{aligned}\Delta a &= A' \cos (kx + c + K\Delta x) - a \\ &= a' \cos k\Delta x + \frac{da'}{dx} \sin \frac{k\Delta x}{R} - a,\end{aligned}$$

which is also the same as before.

"The above therefore is a solution of the equations subject to all its assumptions."

—Not quite. One of those assumptions was that δa was small; but it is evident from (8) that a and δa may attain any magnitude as the time increases (except at certain points at regular intervals in the medium, at which $\cos (kx + c) = 0$); whence it follows that the whole of the preceding investigation is entirely fallacious.

Liverpool, Dec. 1, 1846.

ON PROFESSOR POWELL'S ACCOUNT OF

THE THEORY OF FINITE INTERVALS.

IN the following paper I propose to examine some of the results collected by Professor Powell in his "General and Elementary View of the Undulatory Theory as applied to the Dispersion of Light."* It is with some regret that I have felt obliged to select, as the subject of a not very complimentary criticism, one of the few attempts which Oxford has of late years put forward to vindicate her claim to successful rivalry with her sister university, even in the peculiar line of the latter: but the circumstance of this being the only publication, so far as I am aware, which contains any tolerable account of the different views which have been promulgated on these topics, and the impossibility of referring the ordinary class of readers to the original sources from which Professor Powell has derived the materials of his summary, have compelled me to this course.

The general nature of Professor Powell's views may be surmised from the following paragraph which occurs in his Introduction:—

"The primary object in a mechanical theory is to connect the simple mathematical representation of waves with acknowledged principles derived from dynamics. Such principles were in some degree established by Huyghens, and the more precise dynamical theory of vibrations of an elastic medium, as in the case of sound, investigated by Euler," &c. "was soon seen to apply (at least generally) to the analogous case of undulations of ether constituting *homogeneous* light. But with reference to many peculiar properties, as of *heterogeneous* light, and even in the instance of the propagation of *transverse* vibra-

* London, 1841.

tions (which Young had shewn to be a necessary supposition), further modifications and extensions of the theory were required. Such extension in any degree of generality was first made by Fresnel, principally in his Memoir on Double Refraction, (*Mem. Acad.* Vol. VII.), who clearly pointing out the too limited nature of the ideas of ether hitherto adopted, laid down that of *molecules separated by finite intervals*, by which the whole system of transverse vibrations, of polarization, and the wave surface, were established, by an analysis which must ever remain a monument of the profound and comprehensive genius of its author."

My readers must by this time be tolerably well aware that I entertain a very different opinion of the success of Fresnel's attempt to "extend the theory" from Professor Powell. Into this question, however, I shall not at present enter further than by saying that, whatever may be the merits of Fresnel's "analysis," it is not *reasoning*. It may be *divination*, but it most assuredly does not (to use Professor Powell's somewhat singular phraseology) "connect the simple mathematical representation of a wave with acknowledged principles derived from dynamics;" at least not by any rules of evidence which are ordinarily recognized as such by the human understanding. It is satisfactory, however, to find that Professor Powell is not ashamed of his principles. There are those who, while they give up every step of Fresnel's process, imagine that somehow or other his theory is right as a whole. Professor Powell is more candid if not more discriminating. He contributes his mite of admiration to the process adopted as well as to the inferences deduced.

But although entertaining this high opinion of Fresnel's merits, Professor Powell is not altogether blind to his defects. "The mechanical principles are expounded chiefly in general terms, and the results are not deduced from a mathematical development of equations of motion. Thus these researches, *though of the highest order*, were soon found not to possess in all points that comprehensiveness of principle which would satisfy the increasing demands of discovery; and in some instances the theory was confessedly left defective."—To give an account

of the supplementary hypotheses by which these defects have been sought to be obviated, is a principal object of Professor Powell's work.

“Let the coordinates in space (*vide* Powell on the Undulatory Theory, p. 16) of any molecules $m, m', m'',$ &c. be respectively

$$\begin{aligned} m & \dots\dots x, & y, & z, \\ m' & \dots\dots x + \Delta x, & y + \Delta y, & z + \Delta z, \\ m'' & \dots\dots x + \Delta x', & y + \Delta y', & z + \Delta z', \\ & \text{\&c.} \end{aligned}$$

“When the system is disturbed after a time t , let the displacements be respectively, of

$$\begin{aligned} m & \dots\dots \xi, & \eta, & \zeta, \\ m' & \dots\dots \xi + \Delta\xi, & y + \Delta\eta, & \zeta + \Delta\zeta, \\ m'' & \dots\dots \xi + \Delta\xi', & y + \Delta\eta', & \zeta + \Delta\zeta', \end{aligned}$$

r the distance between m, m' at rest, fr the law of force. Then

$$\begin{aligned} \frac{d^2\xi}{dt^2} &= \Sigma \left\{ mf(r + \Delta r) \frac{\Delta x + \Delta\xi}{r + \Delta r} \right\}, \\ \frac{d^2\eta}{dt^2} &= \Sigma \left\{ mf(r + \Delta r) \frac{\Delta y + \Delta\eta}{r + \Delta r} \right\}, \\ \frac{d^2\zeta}{dt^2} &= \Sigma \left\{ mf(r + \Delta r) \frac{\Delta z + \Delta\zeta}{r + \Delta r} \right\}, \end{aligned}$$

whence we obtain

$$\left. \begin{aligned} \frac{d^2\xi}{dt^2} &= \Sigma m \{ \psi(r) r \Delta r \Delta x + \phi(r) \Delta\xi \} \\ \frac{d^2\eta}{dt^2} &= \Sigma m \{ \psi(r) r \Delta r \Delta\eta + \phi(r) \Delta\eta \} \\ \frac{d^2\zeta}{dt^2} &= \Sigma m \{ \psi(r) r \Delta r \Delta z + \phi(r) \Delta\zeta \} \end{aligned} \right\} \dots\dots (a),$$

[where $\psi(r) = \frac{1}{r} \frac{d}{dr} \left(\frac{fr}{r} \right),$

$$\phi(r) = \frac{fr}{r} + \psi(r) \cdot r \Delta r].$$

With a view to “the integration of these equations” (p. 21)

Professor Powell adopts "a method which, though apparently prolix and capable of simplification, it is yet desirable to follow as it includes other important consequences."

He assumes, in the first place, that the ray is in the direction of the axis, which he conceives may be done "without loss of generality." If the vibration is transversal this implies that

$$\xi = 0, \quad \Delta\xi = 0, \quad \frac{d^2\xi}{dt^2} = 0;$$

so that, observing that

$$r\Delta r = \Delta x\Delta\xi + \Delta y\Delta\eta + \Delta z\Delta\zeta,$$

the equations (a) reduce to

$$\left. \begin{aligned} \frac{d^2\eta}{dt^2} &= \Sigma m \{ \phi(r) \Delta y + \psi(r) \Delta y (\Delta y \Delta\eta + \Delta z \Delta\zeta) \} \\ \frac{d^2\zeta}{dt^2} &= \Sigma m \{ \phi(r) \Delta\zeta + \psi(r) \Delta z (\Delta y \Delta\eta + \Delta z \Delta\zeta) \} \end{aligned} \right\} \dots\dots (\beta).$$

It is next assumed that

$$\left. \begin{aligned} \eta &= \Sigma \{ \alpha \sin (nt - kx) \} \\ \zeta &= \Sigma \{ \beta \sin (nt - kx - b) \} \end{aligned} \right\} \dots\dots (\gamma),$$

which gives us

$$\Delta\eta = \Sigma \alpha \left\{ -2 \sin^2 \frac{k\Delta x}{2} \sin (nt - kx) - \sin k\Delta x \cos (nt - kx) \right\},$$

$$\Delta\zeta = \Sigma \left\{ \begin{aligned} &\beta \cos b \left\{ -2 \sin^2 \frac{k\Delta x}{2} \sin (nt - kx) - \sin k\Delta x \cos (nt - kx) \right\}, \\ &+ \beta \sin b \left\{ + \sin k\Delta x \sin (nt - kx) - 2 \sin^2 \frac{k\Delta x}{2} \cos (nt - kx) \right\}, \end{aligned} \right.$$

$$\text{Also } \frac{d^2\eta}{dt^2} = -m^2 \Sigma \alpha \sin (nt - kx),$$

$$\frac{d^2\zeta}{dt^2} = -n^2 \Sigma \{ \beta \cos b \sin (nt - kx) - \beta \sin b \cos (nt - kx) \}.$$

Substituting these values for $\Delta\eta$, $\Delta\zeta$, $\frac{d^2\eta}{dt^2}$, $\frac{d^2\zeta}{dt^2}$ in equations (β) respectively, and equating to zero the coefficients of $\sin (nt - kx)$ and $\cos (nt - kx)$ in the results, we obtain the four following equations

$$(1) \quad -n^2 \Sigma \alpha = \sin b \Sigma (\beta q \sin 2\theta)$$

$$- \cos b \Sigma (\beta q 2 \sin^2 \theta) - \Sigma (ap 2 \sin^2 \theta),$$

$$(2) \quad 0 = -\Sigma (ap \sin 2\theta) \\ - \cos b \Sigma (\beta q \sin 2\theta) - \sin b \Sigma (\beta q 2 \sin^2 \theta),$$

$$(3) \quad -n^2 \cos b \Sigma \beta = \sin b \Sigma (\beta p' \sin 2\theta) \\ - \cos b \Sigma (\beta p' 2 \sin^2 \theta) - \Sigma (aq 2 \sin^2 \theta),$$

$$(4) \quad -n^2 \sin b \Sigma \beta = -\Sigma (aq \sin 2\theta) \\ - \cos b \Sigma (\beta p' \sin 2\theta) - \sin b \Sigma (\beta p' 2 \sin^2 \theta),$$

[where

$$p = m \{ \phi(r) + \psi(r) \Delta y^2 \},$$

$$p' = m' \{ \phi(r) + \psi(r) \Delta z^2 \},$$

$$q = m \psi(r) \Delta y \Delta z,$$

$$2\theta = k \Delta x].$$

The two last may be replaced by the following,

$$(5) \quad -n^2 \Sigma \beta = -\Sigma (\beta p' 2 \sin^2 \theta) \\ - \cos b \Sigma (aq 2 \sin^2 \theta) - \sin b \Sigma (aq \sin 2\theta),$$

$$(6) \quad 0 = \Sigma (\beta p' \sin 2\theta) - \sin b \Sigma (aq 2 \sin^2 \theta) + \cos b \Sigma (aq \sin 2\theta).$$

Professor Powell next proceeds to draw certain deductions from these equations. He says,

“In the case of elliptic polarization α and β are equal for all the ellipses, so that we have

$$\Sigma \alpha = h\alpha, \quad \Sigma \beta = h\beta,$$

where h is a constant, and we can obtain from (1) and (5),

$$n^2 = \frac{1}{h(\alpha^2 + \beta^2)} \{ \beta^2 \Sigma (p' \sin^2 \theta) + \alpha^2 \Sigma (p \sin 2\theta) \\ + 2\alpha\beta \cos b \cdot \Sigma q 2 \sin 2\theta \};$$

and from (4) we get

$$\sin b = \frac{\alpha \Sigma (q \sin 2\theta) + \beta \cos b \Sigma (p' \sin 2\theta)}{n^2 h \beta - \beta \Sigma (p' 2 \sin^2 \theta)}.$$

From this Professor Powell draws the conclusion that, “upon the whole the formulæ (γ) for elliptically polarized light, involving the above value of n , is the solution of the differential equations (β) for the motion of a system of molecules constituted as at first supposed.”

It is somewhat remarkable that it should not have occurred to Professor Powell that the above values for n^2 and $\sin b$ form but a meagre result from *four* equations. To make my remarks

upon this subject more clear, I must direct attention to an oversight which occurs in the early part of the above process, which throws the whole into almost inextricable confusion. By assuming

$$\eta = \Sigma a \sin (nt - kx),$$

it is to be presumed that Professor Powell meant the symbol Σ to stand for the sum of such a series as the following :

$$(8) \quad a \sin (nt - kx) + a' \sin (n't - k'x) + a'' (\sin n''t - k''x) + \&c.,$$

(and similarly with regard to ξ); but he elsewhere uses the same symbol to indicate a totally different thing, namely, the sum of all the different values assumed by such functions as the following,

$$m \{ \phi (r) + \psi (r) \Delta y^2 \} 2 \sin^2 \frac{k \Delta x}{2} \&c.$$

when different values are assigned to Δx , Δy , Δz .

If we rectify this error by putting

$$\eta = \sigma \{ a \sin (nt - kx) \},$$

$$\zeta = \sigma \{ \beta \sin (nt - kx + b) \},$$

it is obvious that a material change will take place in the nature of the equations (1), (2), (5), (6); for we shall now have from (β)

$$\begin{aligned} \frac{d^2 \eta}{dt^2} = & \{ \sin b \cdot \Sigma (q \sin 2\theta) - \cos b \Sigma (q 2 \sin^2 \theta) \} \times \sigma \{ \beta \sin (nt - kx) \}, \\ & - \{ \cos b \cdot \Sigma (q \sin 2\theta) - \sin b \Sigma (q 2 \sin^2 \theta) \} \times \sigma \{ \beta \cos (nt - kx) \}, \\ & - \Sigma (p 2 \sin^2 \theta) \times \sigma \{ a \sin (nt - kx) \} - \Sigma (p \sin 2\theta) \\ & \times \sigma \{ a \cdot \cos (nt - kx) \}; \end{aligned}$$

and similarly for $\frac{d^2 \zeta}{dt^2}$, η and ζ . But, as Professor Powell assumes that

$$\sigma \beta \sin (nt - kx) = \sin (nt - kx) \sigma \beta,$$

$$\sigma a \sin (nt - kx) = \sin (nt - kx) \sigma a,$$

&c.

it is obvious that we may dispense with the symbol σ altogether, and may take

$$\eta = a \sin (nt - kx), \quad \zeta = \beta \sin (nt - kx + b).$$

Hence, if we put

$$\begin{aligned}\Sigma (p^2 \sin^2 \theta) &= P_1, & \Sigma (p'^2 \sin^2 \theta) &= P'_1, & \Sigma (q^2 \sin^2 \theta) &= Q_1, \\ \Sigma (p \sin 2\theta) &= P_2, & \Sigma (p'_1 \sin 2\theta) &= P'_2, & \Sigma (q \sin 2\theta) &= Q_2,\end{aligned}$$

we may replace equations 1.2.5.6 by the following:

$$(7) \quad \beta (Q_2 \sin b - Q_1 \cos b) - \alpha (P_1 - n^2) = 0,$$

$$(8) \quad \beta (Q_1 \sin b + Q_2 \cos b) + \alpha P_2 = 0,$$

$$(9) \quad \alpha (Q_2 \sin b + Q_1 \cos b) + \beta (P'_1 - n^2) = 0,$$

$$(10) \quad \alpha (Q_1 \sin b - Q_2 \cos b) - \beta P'_2 = 0.$$

Eliminating $\frac{\alpha}{\beta} \cdot b$ and n^2 , from these we shall have a relation between $P_1 P_2, P'_1 P'_2, Q_1 Q_2$, *i.e.* between k and the coordinates of the point of rest of the particles. But k depends on the length of a wave. Hence, upon this principle the length of a wave may be expressed in terms of the coordinates of the medium. Now from the equation thus obtained in PQ , an infinite number of real values of k may be found, but there may be not one. The onus of proof rests with Professor Powell to shew whether any such values can exist. Till this is done it admits of doubt whether "the formula for elliptic polarization involving the above," or any other "value of n , is the solution of the differential equations (β) for the motion of a system of particles constituted as at first supposed."

I next observe that, eliminating $\frac{\alpha}{\beta}$ from (8) and (10), we get

$$(Q_1^2 \sin^2 b - Q_2^2 \cos^2 b) + P_2 P'_2 = 0,$$

$$\text{whence} \quad \sin^2 b = \frac{Q_2^2 - P_2 P'_2}{Q_1^2 + Q_2^2} \dots \dots \dots (11),$$

$$\cos^2 b = \frac{Q_1^2 + P_2 P'_2}{Q_1^2 + Q_2^2} \dots \dots \dots (12).$$

Now $\sin b$ and $\cos b$ are necessarily less than 1. It follows therefore that a relation must exist between $Q_1 Q_2, P_2 P'_2$, in order to satisfy this condition. It rests with Professor Powell to point out from what hypothesis as to the constitution of the medium such a relation would result.

Lastly, eliminating $\frac{a}{\beta}$ from (7) and (8), we get

$$(Q_1^2 \sin^2 b - Q_1^2 \cos^2 b) + (P_1 - n^2)(P'_1 - n^2) = 0,$$

and eliminating b from this by (11) and (12), we have

$$n^2 = \frac{P_1 + P'_1}{2} \pm \sqrt{\left\{ \left(\frac{P_1 - P'_1}{2} \right)^2 + P_2 P'_2 + Q_1^2 - Q_2^2 \right\}}$$

Now admitting $P_1 P'_1$, $P_2 P'_2$, $Q_1 Q_2$ to be all possible, which is not *certain*, as I have shewn, it remains to be proved that n^2 is so. Again, admitting n^2 to be possible, it will in general have *two* values, or we shall have *two waves* propagated with different velocities. It is for Professor Powell to shew how this difficulty can be obviated.

I am aware that these objections may partly be met by supposing a symmetrical disposition of the medium; but Professor Powell repudiates the necessity of any such supposition in the case of elliptic polarization. He says (Art. 105) "the formulas for unpolarized light, as well as the formula for plane polarized light, are *only* solutions, provided" certain "conditions are fulfilled in the original equations. While on the other hand, in the case of elliptically polarized light, it is important to bear in mind that the solution has been obtained solely from the conditions of elliptic polarization."

I shall presently adduce other objections to the case of elliptically polarized in common with that of ordinary light; but in the mean time enough has been said to shew that *it is not true*, that in the case of elliptically polarized light a solution has been obtained *solely from the conditions of elliptic polarization*.

It is obvious from what has gone before that Professor Powell's investigation, although he seems to have persuaded himself to the contrary, relates simply to the case of elliptically polarized light. As his reasoning, however, may be so modified as to apply to the general case, I shall here proceed to give some account of it. Putting $b = 0$, in (1), (2), (3), (4), he obtains four equations, respecting which he observes, that in all of them "it is evident that, since a and β are by the original condition wholly *arbitrary and independent*, both of each other and of the other quantities, these equations can only hold

good for *all values whatever* of a and β , if each of the terms involving respectively a and β are *separately* $= a$."—Now the fact is, there is no reason whatever to suppose that a and β are "wholly independent" of each other, and of the other quantities, or that the equations must hold "for all values whatever of a and β ." If the equations did not hold for some particular values of a and β , it would be no opprobrium (rather the contrary) to the theory; for it would only shew that the medium refuses to transmit rays of a certain character, which we have every reason to believe to be the fact. Passing over the defect of logic however, from the last-named equations Professor Powell deduces the following:

$$\begin{aligned} 0 &= \Sigma (ap \sin 2\theta), & 0 &= \Sigma (\beta q \sin 2\theta), \\ 0 &= \Sigma (\beta p' \sin 2\theta), & 0 &= \Sigma (aq \sin 2\theta), \\ 0 &= \Sigma (\beta q \sin^2 \theta), & 0 &= \Sigma (aq \sin^2 \theta), \end{aligned}$$

$$(13) \quad 0 = n^2 \Sigma a - \Sigma (ap \sin^2 \theta), \quad 0 = n^2 \Sigma \beta - \Sigma (\beta p' \sin^2 \theta) \quad (14),$$

which, although they have in strictness been suggested by the particular case of elliptically polarized light likewise occur in the general case. But in this latter they contain only an imperfect representation of the truth. For taking the series (8) to represent η , it is evident that besides the four equations (1), (2), (5), (6), corresponding to the first term $a \sin (nt - kx)$ of the series, we shall have four exactly similar equations corresponding to every other term as $a' \sin (n't - k'x)$ &c.; in this latter instance $a'\beta'n'k'b'$ recurring in the place of $a\beta nkb$; thus giving us the same equations for the determination of $\frac{a'}{\beta'}$, n' , k' , β'

as for $\frac{a}{\beta}$, n , k , b . If we choose to satisfy these independently of $\frac{a}{\beta}$, we must have

$$P_2 = P'_2 = Q_1 = Q_2 = 0, \quad P_1 - n^2 = 0, \quad P'_1 - n'^2 = 0.$$

Professor Powell afterwards enters (Art. 108, *et seq.*) into some considerations to shew how the first four of these equations may be satisfied by "an hypothesis respecting the *ethereal molecules* in space, viz. that they are *distributed uniformly*."

Now I admit that on this hypothesis we satisfy the equations $P_2 = 0$, $P'_2 = 0$, $Q_2 = 0$. But the case of $Q_1 = 0$, or

$$\Sigma m \psi(r) \Delta y \Delta z \cdot 2 \sin^2 \frac{k \Delta x}{2}$$

presents more difficulty. Professor Powell says, whenever the three first "are evanescent it is easy to shew that

$$\Sigma m \psi(r) \Delta y \Delta z \cdot 2 \sin^2 \frac{k \Delta x}{2} = 0,$$

by a simple transformation of coordinates:" and then assuming

$$y = y' \cos t - z' \sin t,$$

$$z = z' \cos t + y' \sin t,$$

he shews that, since (putting $P = \psi(r) \cdot 2 \sin^2 \theta$)

$$\Sigma (P \Delta y \Delta z) = \{ \Sigma (P \Delta y'^2) - \Sigma (P \Delta z'^2) \} \sin 2\theta - 2 \Sigma (P \Delta x' \Delta y') \cos 2\theta,$$

we may, by properly assuming $\tan 2\theta$, cause $\Sigma (P \Delta y \Delta z)$ to vanish.

I must here, however, recall to Professor Powell's recollection a circumstance which seems to have escaped him. When we

assume that ξ , $\Delta \xi$, and $\frac{d^3 \xi}{dt^2}$ all = 0, the first of the equations (a)

requires certain conditions to be satisfied. It is evident, in fact, that in this case we must have

$$(15) \quad 0 = \Sigma m \{ \psi(r) \Delta x (\Delta y \Delta \eta + \Delta z \Delta \zeta) \},$$

which, substituting for $\Delta \eta$ and $\Delta \zeta$ their values, implies the four following equations,

$$0 = \Sigma m \psi(r) \Delta x \Delta y \cdot 2 \sin^2 \frac{k \Delta x}{2},$$

$$0 = \Sigma m \psi(r) \Delta x \Delta y \cdot 2 \sin k \Delta x,$$

$$0 = \Sigma m \psi(r) \Delta x \Delta z \cdot 2 \sin^2 \frac{k \Delta x}{2},$$

$$0 = \Sigma m \psi(r) \Delta x \Delta z \sin k \Delta x.$$

The second and fourth of these may be satisfied by the hypothesis of uniform distribution, but the first and third cannot, and the only way of getting over this difficulty is by transforming the coordinates. Hence it is evident that, instead of

a simple transformation in the plane of yz , an entire transformation of the system must be effected, in order to satisfy the three conditions,

$$0 = \Sigma (P\Delta y\Delta z), \quad 0 = \Sigma (P\Delta x\Delta z), \quad 0 = \Sigma (P\Delta y\Delta x).$$

I believe it will be found that there is only *one* position of the axes which will satisfy this condition, as M. Cauchy, who appears in this instance to have been rather clearer sighted than Professor Powell, admits (*vid.* Powell, Art. 141): and as it has been assumed that the ray coincides in direction with the axis of x , we are thus led to the somewhat astounding conclusion that there are *only three directions*, at right angles to one another, in which a wave of given length can be propagated.—Truly this is having ‘axes of elasticity’ with a vengeance.

But this is not all. It will be seen that the equations (13), (14), give us two values of n^2 , namely P_1 and P'_1 . Professor Powell considers these as identical (*vid.* Art. 97), and this apparently without any reference to the nature of the medium. For my own part I confess myself unable to conceive how this could occur unless we suppose the particles to be arranged in square order, and that the ray is propagated along one of the sides of the square.—But even this wretched fragment of a result (if any one should think it deserving the least attention) will be found to rest on no solid basis. Recurring to the equations (a) it is obvious that the assumptions

$$\xi = 0, \quad \Delta\xi = 0, \quad \frac{d^2\xi}{dt^2} = 0,$$

involve some further conditions beyond those involved in the equation (15). We have

$$\frac{d^2\xi}{dt^2} = \Sigma m \left\{ \frac{fr}{r} + \frac{d}{dr} \cdot \frac{fr}{r} \cdot \frac{\Delta r}{1} + \frac{d^2}{dr^2} \cdot \frac{fr}{r} \cdot \frac{\Delta r^2}{1.2} + \&c. \right\} (\Delta x + \Delta\xi).$$

Now, in order that $\frac{d^2\xi}{dt^2}$ may = 0, besides the equations

$$0 = \Sigma m \frac{fr}{r}, \quad 0 = \Sigma m \left\{ \frac{d}{dr} \cdot \frac{fr}{r} \cdot \Delta x \Delta r + \frac{fr}{r} \cdot \Delta\xi \right\},$$

[the second of which is identical with (15)], we must have

$$0 = \Sigma m \left\{ \frac{d^2}{dr^2} \cdot \frac{fr}{r} \cdot \frac{\Delta r^2}{1.2} \cdot \Delta x + \frac{d}{dr} \left(\frac{fr}{r} \cdot \frac{\Delta r}{1} \Delta \xi \right) \right\};$$

and similarly of every other term in the equivalent of $\frac{d^2 \xi}{dt^2}$. It is

obvious that these conditions cannot be satisfied by the hypothesis of uniform distribution, whence it follows that there will be an accelerating force on the particle parallel to the axis of x . Now, though this may be small, its effects in a given time may be very important; as, by means of it, ξ instead of being 0, or even very small, may come to have any assignable magnitude. It is incumbent therefore on the supporters of this theory to shew that the variations of ξ will be confined between small limits.—I apprehend that this object can hardly be effected without breaking up the entire theory.

Having thus considered the theory which Professor Powell has treated of most in detail, I shall say a few words with regard to the “other solutions of the Differential Equations,” of which he gives an account in his 4th Section. The first of these is Mr. Kelland's,* who “deduces, as above, the fundamental equations. Then developing $\Delta \xi$, $\Delta \eta$, $\Delta \zeta$, each as functions of the three variables x, y, z , we have

$$\begin{aligned} \Delta \xi = & \frac{d\xi}{dx} \Delta x + \frac{d\xi}{dy} \Delta y + \frac{d\xi}{dz} \Delta z, \\ & + \frac{d^2 \xi}{dx^2} \frac{\Delta x^2}{2} + \frac{d^2 \xi}{dy^2} \frac{\Delta y^2}{2} + \frac{d^2 \xi}{dz^2} \frac{\Delta z^2}{2}, \\ & + \frac{d^2 \xi}{dx dy} \Delta x \Delta y + \frac{d^2 \xi}{dx dz} \Delta x \Delta z + \frac{d^2 \xi}{dy dz} \Delta y \Delta z + \&c., \end{aligned}$$

and similar expressions for $\Delta \eta$ and $\Delta \zeta$.” Then “adopting the hypothesis of symmetrical distribution, and thus rejecting the sums of products of odd dimensions in Δx , Δy , Δz ; rejecting also products of the same terms of four dimensions as insensibly small,” he reduces the equations to an integrable form.

But it is obvious that this process is entirely fallacious, as

* Mr. Kelland appears to have subsequently abandoned this theory, and to have adopted that which I have discussed in the preceding paper.

depending upon a false approximation. Instead of Δx , Δy , Δz being so small that products involving the fourth powers of those quantities may be neglected as insensible, these quantities may have any magnitude whatever. This is so obvious that I cannot but feel some surprise that such a method should ever have been proposed.

Mr Kelland, however, is not singular in this error. "Mr. Tovey (in his *first* papers in the *Journal of Science*, Vol. VIII. pp. 7, 270, 500)," as Professor Powell informs us, "after establishing the fundamental equations, proceeds to develop ξ , η , ζ as each a function of Δx . He thus finds

$$\Delta \xi = \frac{d\xi}{dx} \Delta x + \frac{d^2\xi}{dx^2} \frac{\Delta x^2}{1.2} + \&c.,$$

and similar expressions for $\Delta \eta$ and $\Delta \zeta$."—Now we can make no use of this series for $\Delta \xi$, for, in the first place, as Δx may have any magnitude, we are not sure that even the whole series would be an equivalent for $\Delta \xi$, and *à fortiori* we cannot take a limited number of its terms to represent that quantity.—

"Sir John Lubbock (*Journal of Science*, Vol. XI. Nov. 1837) developing $\Delta \rho$ as a function of r ,

$$\Delta \rho = \frac{d\rho}{dr} \Delta r + \frac{d^2\rho}{dr^2} \cdot \frac{\Delta r^2}{1.2} + \&c.,"$$

obtains the following equations

$$\begin{array}{ccc} \frac{d^2\xi}{dt^2} = a^2 \cos X \frac{d^2\rho}{dr^2} + a^2 \cos X \frac{d''\rho}{dr^4} . \\ \&c. & \&c. \end{array}$$

But this is evidently untenable as depending on the same false approximation as before, since Δr is *not* small. Professor Powell makes an elaborate comparison between Sir John Lubbock's results and those of Fresnel in connexion with the axes of elasticity, and seems to think that the former correspond with the latter. The fact therefore of Sir John Lubbock's process being erroneous is in complete accordance with my views as to that of Fresnel.

It would be beside my purpose to enter into the consideration of Professor Powell's formulæ for the 'diffusion'; for, as

those formulæ have not upon theoretical grounds the smallest claim on our attention, the only result of such a step would be to expose, possibly, further error and inconsistency. If Professor Powell thinks his formulæ are to be defended on experimental grounds, there are those who are both able and willing to combat them upon those grounds. In this respect, however, I would warn my readers against the too easy reception of boldly asserted claims, and I would recommend them to judge for themselves of the probability of formulæ thus arrived at, giving any real exact representation of the phenomena of nature. The formulæ of Fresnel and his followers may truly represent the phenomena of light, and so *may* some of those given in Dr. Peacock's Examples on the Differential and Integral Calculus; but as there appears to be no other ground for preferring the former to the latter beyond that of ascertained accordance with experiment, it appears to be necessary to sift with the utmost keenness the evidence which may be adduced with regard to this point.

Liverpool, Jan. 6, 1847.

ON MR. GREEN'S THEORY OF

REFLECTION AND REFRACTION.

THE late Mr. George Green, of Caius College, in a paper "On the Laws of Reflection and Refraction of Light at the common surface of two non-crystallized Media" (read before the Cambridge Philosophical Society, Dec. 11, 1837), appears, as far as I am able to judge from a very cursory inspection, to have arrived at some conclusions respecting the intensity of reflected and refracted light, which agree more or less with those of Fresnel. How operations conducted apparently upon different principles should, though equally erroneous, lead, to a certain extent, to similar results, it is not my purpose to explain, though I apprehend that in this instance it would not be difficult to do so. It is, for the present, sufficient to shew, as I am prepared to do, that Mr. Green's results, whatever they are, are based upon a false analysis.

The explanation of Mr. Green's theory, as far as is necessary, I shall give in his own words.

"Let us conceive a mass composed of molecules acting on each other by any kind of molecular forces, but which are sensible only at insensible distances, and let moreover the whole system be quite free from all extraneous action of every kind. Then, x, y, z being the coordinates of any particle of the medium under consideration when in equilibrium, and

$$x + u, \quad y + v, \quad z + w,$$

the coordinates of the same particle in a state of motion [where u, v, w are very small functions of the original coordinates (x, y, z) of any particle and of the time (t)], we get, by combining D'Alembert's principle with that of virtual velocities,

$$\Sigma \Delta m \left(\frac{d^2 u}{dt^2} \delta u + \frac{d^2 v}{dt^2} \delta v + \frac{d^2 w}{dt^2} \delta w \right) = \Sigma Dv \cdot \delta \phi \dots (1),$$

Dm and Dv being exceedingly small corresponding elements of the mass and volume of the medium, but which nevertheless contain a great number of molecules, and $\delta\phi$ the exact differential of some function and entirely due to the internal actions of the particles of the medium on each other

“Let us now take any element of the medium, rectangular in a state of repose, and of which the sides are dx, dy, dz , the length of the sides composed of the same particles will in a state of motion become

$$dx' = dx (1 + s_1), \quad dy' = dy (1 + s_2), \quad dz' = dz (1 + s_3)$$

where s_1, s_2, s_3 are exceedingly small quantities of the first order. If, moreover, we make

$$\alpha = \cos < \frac{dy'}{dz'}, \quad \beta = \cos < \frac{dx'}{dz'}, \quad \gamma = \cos < \frac{dx'}{dy'},$$

α, β, γ will be small quantities of the same order. But whatever may be the nature of the internal actions, if we represent by

$$\delta\phi \, dx \, dy \, dz$$

the part of the second member of the equation (1), due to the molecules in the element under consideration, it is evident that ϕ will remain the same when all the sides and all the angles of the parallelopiped, whose sides are dx', dy', dz' , remain unaltered, and therefore its most general value must be of the form

$$\phi = \text{function} (s_1 \, s_2 \, s_3, \alpha \, \beta \, \gamma).$$

But $s_1, s_2, s_3, \alpha, \beta, \gamma$ being very small quantities of the first order, we may expand ϕ in a very convergent series of the form

$$\phi = \phi_0 + \phi_1 + \phi_2 + \phi_3 + \&c.,$$

$\phi_0, \phi_1, \phi_2, \&c.$ being homogeneous functions of six quantities $\alpha, \beta, \gamma, s_1, s_2, s_3$ of the degrees 0, 1, 2, $\&c.$, each of which is very great compared with the next following one. If now, ρ represent the primitive density of the element $dx \, dy \, dz$, we may write $\rho \, dx \, dy \, dz$ in the place of Dm in the formula (1), which will thus become, since ϕ_0 is constant,

$$\begin{aligned} & \iiint \rho \, dx \, dy \, dz \left\{ \frac{d^2 u}{dt^2} \delta u + \frac{d^2 v}{dt^2} \delta v + \frac{d^2 w}{dt^2} \delta w \right\} \\ & = \iiint dx \, dy \, dz (\delta\phi_1 + \delta\phi_2 + \&c.) \end{aligned}$$

the triple integrals extending over the whole volume of the medium under consideration.

“But by the supposition, when $u = 0$, $v = 0$, $w = 0$, the system is in equilibrium, and hence

$$0 = \iiint dx \, dy \, dz \, \delta\phi,$$

seeing that ϕ_1 is a homogeneous function of $s_1, s_2, s_3, \alpha, \beta, \gamma$ of the first degree only. If therefore we neglect ϕ_3, ϕ_4 &c., which are exceedingly small compared with ϕ_2 , our equation becomes

$$\begin{aligned} \iiint \rho \, dx \, dy \, dz \left\{ \frac{d^2u}{dt^2} \delta u + \frac{d^2v}{dt^2} \delta v + \frac{d^2w}{dt^2} \delta w \right\} \\ = \iiint dx \, dy \, dz \, \delta\phi_2, \end{aligned}$$

the integrals extending over the whole volume under consideration.

“If now we can obtain the value of ϕ_2 , we shall only have to apply the general methods given in the *Mécanique Analytique*. But ϕ_2 being a homogeneous function of six quantities of the second degree will in its most general form contain twenty-one arbitrary coefficients. The proper value to be assigned to each will of course depend on the internal constitution of the medium. If however the medium be a non-crystallized one, the form of ϕ_2 will remain the same, whatever be the directions of the coordinate axes in space. Applying this last consideration, we shall find that the most general form of ϕ_2 for non-crystallized bodies contains only two arbitrary coefficients. In fact, by neglecting quantities of the higher orders, it is easy to perceive that

$$\begin{aligned} s_1 = \frac{du}{dx}, \quad s_2 = \frac{dv}{dy}, \quad s_3 = \frac{dw}{dz}; \\ \alpha = \frac{dw}{dy} + \frac{dv}{dz}, \quad \beta = \frac{dw}{dx} + \frac{du}{dz}, \quad \gamma = \frac{du}{dy} + \frac{dv}{dz}; \end{aligned}$$

and if the medium is symmetrical with regard to the plane (xy) only, ϕ_2 will remain unchanged when $-z$ and $-w$ are written for z and w . But this alteration evidently changes α and β to $-\alpha$ and $-\beta$. Similar observations apply to the planes $(xz), (yz)$.

“If therefore the medium is merely symmetrical with respect

to each of the coordinate planes, we see that ϕ_2 must remain unaltered when

$$\begin{array}{l} \text{or} \\ \text{or} \end{array} \quad \left. \begin{array}{l} -z, -w, -\alpha, -\beta, \\ -y, -v, -\alpha, -\gamma, \\ -x, -w, -\beta, -\gamma \end{array} \right\} \text{ are written for } \left\{ \begin{array}{l} z, w, \alpha, \beta, \\ y, v, \alpha, \gamma, \\ x, w, \beta, \gamma. \end{array} \right.$$

“In this way the twenty-one coefficients are reduced to nine, and the resulting function is of the form

$$\begin{aligned} G \left(\frac{du}{dx} \right)^2 + H \left(\frac{dv}{dy} \right)^2 + I \left(\frac{dw}{dz} \right)^2 + La^2 + M\beta^2 + N\gamma^2 \\ + 2P \frac{dv}{dy} \cdot \frac{dw}{dz} + 2Q \frac{du}{dx} \cdot \frac{dw}{dz} + 2R \frac{du}{dx} \cdot \frac{dv}{dy} = \phi_2. \end{aligned}$$

“Probably the function just obtained may belong to those crystals which have three axes of elasticity at right angles to each other.”

Mr. Green then proceeds still further to simplify the equations, preparatory to the discussion of the case which he has made the object of his analysis, viz. of reflection and refraction in non-crystallized media. But unfortunately the simplification which has already been made is altogether untenable. Mr. Green talks of the medium being *symmetrical*. How is it possible that a medium can be symmetrical in a state of motion? We are at perfect liberty to make use of the consideration of symmetry as regards the coordinates (x, y, z) of the points of rest of the particles, but most assuredly the same principle cannot be extended to the variable coordinates (u, v, w) of the particles in a state of motion, and yet it is nothing short of this that Mr. Green's theory requires. The nearest approach which I can imagine to a state of symmetry when there is motion, is in the case of a series of indefinitely extended plane waves advancing perpendicularly to their points. But this is, properly speaking, *regularity* not *symmetry*,—most certainly not that kind of symmetry which Mr. Green contends for, and upon which his simplification of the equation is based. Moreover, the motion we are now considering is not an *actual* motion which we are at liberty to assume of such simple character as may suit our convenience, but an *arbitrary geometrical one*;

a motion in fact which is restricted solely by the conditions of *being small* and of *preserving the continuity of the medium*. There is not the smallest reason for imagining the slightest relation between the motions in opposite quadrants, beyond what the 'equation of continuity' may afford. If any one shall be found of sufficient force of thought to wrest from that equation the fact of such a symmetrical arrangement of the medium being necessary, as Mr. Green's theory implies, then I shall be ready to admit that gentleman's simplification of his equation; but till then I must contend that *that simplification is altogether inadmissible, and that every result founded upon it must be entirely rejected*.

February 13, 1847.

ON PROFESSOR MACCULLAGH'S THEORY.

IN a paper read to the Royal Irish Academy, Dec. 9, 1839, and published in the Transactions of that body, Professor MacCullagh enters upon a somewhat different branch of the wave theory from that, the discussion of which by Mr. Green we have just been considering. The latter gentleman, as may be recollected, confines his attention to the Reflection and Refraction of Light in non-crystallized media. The object of Professor MacCullagh, on the other hand, is to make an "Essay towards a Dynamical Theory of Crystalline Reflection and Refraction." But the results of Professor MacCullagh, equally with those of Mr. Green, are entirely erroneous, the error in the two cases being so far of the same character as that each consists in a false simplification of the same equation. It is not my intention to occupy the time of my readers with vain regrets at the waste of ingenuity displayed in this theory—a waste arising, as far as I am able to judge after a very cursory examination, from a single inadvertence, which in its turn owes its origin to the besetting sin of Fresnel and his followers—the attempting to establish a foregone conclusion by the pure force of analysis. Unlike every other writer on the undulatory theory whom I have met with, Professor MacCullagh appears to be fully aware of the "altogether unsatisfactory" nature of Fresnel's theory; but he appears to have been dazzled by the brilliant results which that theory professed to elicit, and seems to have come to the conclusion that, whatever its defects, somehow or other, Fresnel had hit upon the right track; and that all that was requisite was to establish a firm communication between the ordinary regions of philosophy and the species of flying island which the ingenious author of the theory, of double refraction appeared to have brought into existence.—But it is

time that my readers should have an opportunity of deciding for themselves upon the justice of these strictures, and therefore without further preamble I shall proceed to give an account of Professor MacCullagh's theory.

"The assumptions on which the theory rests are these: *First*, that the density of the luminiferous ether is a constant quantity; in which it is implied that this density is unchanged either by the motions which produce light or by the presence of material particles, so that it is the same within all bodies as in free space, and remains the same during the most intense vibrations. *Second*, that the vibrations in a plane wave are rectilinear, and that, while the plane of the wave moves parallel to itself, the vibrations continue parallel to a fixed right line; the direction of this right line and the direction of the normal to the wave being functions of each other. This supposition holds in all known crystals except quartz, in which the vibrations are elliptical.

"Concerning the peculiar constitution of the ether we know nothing and shall suppose nothing, except what is involved in the foregoing assumptions. But with respect to its physical condition generally, we shall admit, as is most natural, that a vast number of ethereal particles are contained in a differential element of volume; and, for the present, we shall consider the mutual actions of these particles to be sensible only at distances which are insensible when compared with the length of a wave.

"Let xyz be the rectangular coordinates of a particle before it is disturbed, and $x + \xi$, $y + \eta$, $z + \zeta$ its coordinates at the time t , the displacements ξ , η , ζ being functions of x , y , z and t . Let the ethereal density, which is the same in all media, be regarded as unity, so that $dx dy dz$ may, at any instant, represent indifferently either the element of volume or of mass. Then the equation of motion will be of the form

$$(1) \iiint dx dy dz \left(\frac{d^2 \xi}{dt^2} \delta \xi + \frac{d^2 \eta}{dt^2} \delta \eta + \frac{d^2 \zeta}{dt^2} \delta \zeta \right) = \iiint dx dy dz \delta V,$$

where V is some function depending on the mutual actions of the particles. The integrals are to be extended over the whole volume of the vibrating medium, or over all the media if there be more than one.

"We come now to investigate the particular form which must be assigned to the function V , in order that the formula (1) may represent the motions of the ethereal medium. For this purpose conceive the plane of $x' y'$ to be parallel to a system of plane waves whose vibrations are entirely transversal and parallel to the axis of y' , so that $\xi' = 0$, $\zeta' = 0$. Imagine an elementary parallelopiped $dx' dy' dz'$, having its edges parallel to the axes of x' , y' , z' , to be described in the ether when at rest, and then all its points to move according to the same law as the ethereal particles which compose it. The faces of the parallelopiped which are perpendicular to the edge dz' will be shifted, each in its own plane, in a direction parallel to the axis of y' , but their displacements will be unequal and will differ by dy' , so that the edges connecting their corresponding angles will no longer be parallel to the axis of z' , but will be inclined to it at an angle κ whose tangent is $\frac{dy'}{dz'}$.

"Now the function V can only depend on the directions of the axes of $x' y' z'$ with respect to fixed lines in the crystal, and upon the angle κ which measures the change of form produced in the parallelopiped by vibration. This is the most general supposition which can be made concerning it. Since however, by our second assumption, any one of these directions, suppose that of x' , determines the other two, we may regard V as depending on the angle κ and on the direction of the axis of x' alone. But" it may be shewn "that the angle κ and the angles which the axis of x' makes with the fixed axes of x, y, z , are all known when the quantities X, Y, Z are known. Consequently V is a function of $X Y Z$."

On this basis it is not difficult to reduce δV to the form

$$- \delta V = a^2 X dX + b^2 Y dY + c^2 Z dZ,$$

but unfortunately this basis is entirely erroneous.

It is a mistake to suppose that "the function V can only depend on the directions of the axes of $x' y' z'$ with respect to fixed lines in the crystal and upon the angle κ which measures the change of form produced in the parallelopiped by vibration." It is not "the change of form produced in the parallelopiped by" any actual "vibration," but that which arises from the *arbitrary geometrical motion* which the "methods

given in the *Mécanique Analytique* contemplate. It is not true that the hypothesis as to V depending on the directions of the axes of $x' y' z'$ with respect to fixed lines in the crystal and upon the angle u ," is the most general supposition which can be made. The following paragraphs may serve to prove to those who have any doubt on the point what were the views of Lagrange with regard to it.

"Nous nommerons dans la suite* (vide *Mécan. Anal.* p. 196) ces différentielles marquées par δ , des *variations* pour les distinguer des autres marquées par d qui se trouvent dans la même formule, et qui expriment les accroissemens ou décroissemens successifs des variables, à raison du tems et du mouvement des corps; tandis que les *variations* sont relatives au changement instantanée des corps, et qui est tout à fait indépendant de leur mouvement effectif.

"Ensuite en ayant égard aux équations de condition, données par la nature du système proposé, entre les coordonnées des différens corps, on reduira les *variations* de ces coordonnées au plus petit nombre possible, ensorte que les variations restantes soient tout-à-fait indépendantes entr'elles et absolument arbitraires. Alors on égalera à zero la somme de tous les termes affectés de chacune de ces dernières variations; et l'on aura toutes les equations nécessaires pour la determination du mouvement du système."

Now what are the equations of condition in the present case? Clearly they reduce themselves to one, the equation of incompressibility. Professor MacCullagh has introduced the hypothesis that the medium is incompressible, and this (however improbable in itself) for the purpose of discussion is perfectly allowable. But it is not allowable to introduce the further assumption, which Professor MacCullagh in effect does introduce, that the motion is *constrained* to take place in the direction of certain fixed lines. It is one thing to suppose that a series of plane waves *may* be propagated in which the vibration is rectilinear and transversal, and another to assume that the medium is incapable of any other kind of motion.

It is not, however, altogether without reflection that Professor MacCullagh has fallen into this error. The point seems to

* Paris, 1788.

have been brought before him, although he has arrived at a wrong conclusion respecting it. He says (vide p. 4):

“By putting together the assumptions we have made, it will appear that when a system of plane waves disturbs the ether, the vibrations are transversal, or parallel to the plane of the waves. For all the particles situated in a plane parallel to the waves are displaced from their positions of rest through equal spaces in parallel directions; and therefore, if we conceive a closed surface of any form, including any volume great or small, to be described in the quiescent ether, and then all its points to partake of the motion imparted by the waves, any slice cut out of that volume, by a pair of planes parallel to the wave plane and indefinitely near each other, can have nothing but its thickness altered by the displacements: and since the assumed preservation of density requires that the volume of the slice should not be altered, nor consequently its thickness, it follows that the displacement must be in the plane of the slice, that is to say, they must be parallel to the wave plane.”

But this, although a fair account of the *actual* motion (if any motion of the kind supposed could exist under the assumptions which are made) has nothing to do with the *arbitrary* motion with which we have now to deal, which is quite independent of the actual movement. The only conditions to which the medium is subject are those of continuity and incompressibility. If Professor MacCullagh thinks that from those conditions it follows that the portion of the function V due to the element dx', dy', dz' is independent of μ, ν (the other two angles into which the parallelopiped may be forced), and depends solely on the directions of $x' y' z'$ and the angle κ , let him prove it: but I must observe that he has not done this yet, and until he has done so I am under the necessity of affirming that—his system is erroneous, and his results inadmissible.*

February, 13, 1847.

* I may observe that the existence of two transversal waves polarized at right angles to each other, which Professor MacCullagh deduces from his theory, is precisely what, under such premises, might have been expected to occur; for the condition that the motion is *constrained* to take place in a direction transverse to the course of the wave, is what actually occurs in the case of a *stretched cord*, so that we are necessarily led to the equations applying to that case.

REPLY TO SOME REMARKS OF MR. AIRY

Contained in a Recent No. of the Philosophical Magazine.

SINCE the publication of my two papers on Double Refraction, the individual who is generally looked upon as at once the most zealous and the most able champion of Fresnel's theory in this country, has thought proper to issue a manifesto expressing the nature of his faith therein, which, as indicating the grounds upon which the controversy must for the future be conducted, I shall now lay before my readers. It is contained in a paper inserted in the *Philosophical Magazine* (No. 189, p. 469), entitled, "On the Equations applying to Light under the action of Magnetism," in which Mr. Airy states as follows:—

"In order to justify my intruding upon you with a suggestion which is exceedingly imperfect, I think it right to state to you my opinion upon the present state of the optical theory, and upon several steps which, though leading to nothing conclusive, have nevertheless contributed to the real intellectual progress of science.

"Of the truth of the undulatory theory, as regards the geometrical representation of light by undulations based upon transversal vibrations, the resolution of which into vibrations at right angles to each other constitutes polarization, I have not the shadow of a doubt. These undulations, whatever may be the way in which they may have been originally created, I conceive to be propagated by mechanical laws applying to the attractive or repulsive forces of the particles of the medium, the assumed æther, or the medium and the æther combined. But I have seen no mechanical theory to which I attach much importance or any unqualified belief. Nevertheless I think that the investigation and publication of these mechanical theories have been

advantageous to the science by shewing that mechanical laws *may be able* to explain effects never before ascribed to mechanical laws. As regards the progress of intellect it has been very important to shew that variation of velocities, as depending on the period of the oscillations, is mechanically possible ; it has been very important to shew that transversal vibrations are mechanically possible ; it has been very important to shew that crystalline separation of differently polarized rays is mechanically possible. It is not that I believe completely in any one of the mechanical explanations which have been given, but that *à priori* difficulties have been removed, and that it may now be considered that there is a fair chance of reducing the whole to mechanical explanation."

This is Mr. Airy's present confession of faith. It is satisfactory to find that in the course of fifteen years he has made some small progress towards a just appreciation of Fresnel's theory. In 1831 we find him, in the preface to the 2nd edition of his *Tracts*, making the following statement. "The *mechanical* part of the theory, as the suppositions relative to the constitution of the ether, the computation of the intensity of reflected and refracted rays, &c., though generally probable. I conceive to be far from certain." Thus, while in 1831 Mr. Airy, although conceiving the mechanical part of the theory to be far from certain, thought it 'generally probable,' in 1846 he declares he has seen no theory to which 'he attaches much importance,' much less 'unqualified belief.' I am afraid that this confession says very little for the progress of the theory in the mean time ; but be this as it may, I am ready to accept the controversy exactly as Mr. Airy has proposed it.

Mr. Airy thinks that "the investigation and publication of these mechanical theories have been advantageous to the science, by shewing that mechanical laws *may be able* to explain effects never before ascribed to mechanical laws." Has this been done ? Have the mechanical theories of which Mr. Airy speaks been even of the limited utility he ascribes to them ? Mr. Airy thinks it 'very important to shew that the variation of velocities, as depending on the period of the vibrations, is mechanically possible' To what single investigation will Mr. Airy point as

effecting this purpose? In the last edition of Mr. Airy's own Tract on the Theory of Light, we find an investigation professing to have accomplished this object: but independently of the preposterous absurdity of the process adopted, the result obtained is altogether untenable; for if that result were true, it would follow that we should have dispersion in the free ether just as much as in a mixed medium—an idea which cannot be entertained for a moment, and which Mr. Airy himself would be the last to advocate.

Possibly Mr. Airy, like Sir John Herschel, thinks it enough that a phenomenon be explained on a certain hypothesis, even though we are perfectly certain that that hypothesis has no foundation in fact.

In the 'Treatise on Light' (*Encyc. Met.* Art. 989) we find the following extraordinary specimen of ratiocination.

"Let us take with M. Fresnel (*Annales de Chimie*, xvii. p. 179, *et seq.*) the case of a crystal with one axis. We may regard this, or rather the ether within it, modified in its action by the molecular forces of the crystal, as an elastic medium in which the elasticity in a direction perpendicular to the axis is different from that in a direction parallel to it, that is, in which the molecules are more easily compressible in the one than in the other direction; but equally so in all directions perpendicular to the axis, on whatever side the pressure be applied. To aid our conceptions in imagining such a property, we may assimilate an uniformly elastic medium to an assemblage of thin, elastic, hollow, spherical shells in contact; and such a medium as we are considering to a similar assemblage of oblate or prolate hollow ellipsoids, arranged with all their axes parallel to one common direction, which is that of the axis of the crystal. It is evident that the resistance of the spherical assemblage to pressure must be the same in all directions, but that of the spheroidal must differ according as the pressure is applied perpendicularly or parallel to the axis. Thus, it is easy to crush an egg by a force applied in the direction of its shorter diameter, which will yet sustain a violent pressure applied at the extremities of its longer. It is moreover evident, if any molecule of such an assemblage were disturbed so as

to throw it into vibration, that, provided always the amplitude of its excursions were extremely small compared to the diameter of each ellipsoid, the immediate tendency of the vibration will be to communicate motion to two strata only of molecules, viz. that in which the axis and equator of the disturbed molecule lie respectively, since it is only at the poles and equator that they touch, and therefore only through these points that motion can be communicated. Consequently, any motion communicated to a molecule of such a mass could only be propagated by vibrations performed in planes parallel and perpendicular to the axis. Hence, if a vibratory motion in any plane be propagated into such an assemblage of particles from without, it will immediately, on its reaching it, be resolved into two in the planes aboved named, and these, by reason of the different elasticities, will be propagated with different velocities.

“The reader must not suppose that this is intended for an account of the real mechanism of crystallized bodies. *It is merely intended to shew that it is not absurd or contrary to sound mechanical principles, to assume that such MAY be their constitution, that vibrations can be propagated through them by molecular excursions executed in planes parallel and perpendicular to their axes.*”

Such arguments may be current in the skies with which Mr. Airy and Sir John Herschel are familiar, but they certainly are not such as commonly circulate in this lower sphere. I have elsewhere expressed myself at a loss to understand the support which a theory can derive from an explanation which no one believes. Mr. Airy and Sir John Herschel however appear to be of a different opinion.

In the same note to the second edition which I have before adverted to, Mr. Airy refers his readers to Professor Powell's work for further explanation as to the doctrine of different refrangibility. I have in an earlier part of this work examined each of the methods treated of by Professor Powell, as also some others which have been proposed from different quarters, and I have shewn that they are all alike erroneous. I assert therefore, and I challenge Mr. Airy to gainsay the assertion,

that, *it is not true* "that the variation of velocities, as depending on the period of the vibration, has been shewn to be mechanically possible."

Let us now, however, turn to the second declaration of Mr. Airy. He says, "It has been very important to shew that transversal vibrations are mechanically possible." Again I ask has this been done? In Mr. Airy's own work we find an investigation having for its object to shew the possibility of transversal vibrations, which investigation I have shewn to be entirely erroneous. If Mr. Airy relies on Professor Powell, or Mr. Kelland, or Mr. Green, or Professor MacCullagh for aid in this particular, I must beg to assure him that the processes adopted by those gentlemen are as fallacious as his own, and that *they* fail as completely as he does himself to demonstrate that transversal vibrations are possible.

Lastly, Mr. Airy considers, "it has been very important to shew that the separation of differently polarized rays is mechanically possible." Does Mr. Airy allude to Fresnel's theory? I think I may without vanity presume that my attack on that theory was the immediate cause of Mr. Airy's present declaration: but I cannot flatter myself that he has done me the honour of reading my papers—perhaps he has been too much en-"gaged." But whatever the cause, as the fact appears certain, I must trouble Mr. Airy with a brief recapitulation of the objections which may be urged against Fresnel's theory of the separation of the ray. 1. In the first place, then, there are no axes of elasticity such as Fresnel has supposed. 2. If these last existed, there would be no undulation. 3. Granting all Fresnel's postulates there would be no polarization. The motion of the particles in a plane perpendicular to the point of the wave is, even upon his own principles, clearly impossible. These positions suffice to establish that it is *not true* that Fresnel's hypothesis shews "that the separation of differently polarized rays is mechanically possible."

One cannot but admire the rhetorical skill with which Mr. Airy endeavours to acquire the support of every investigation connected with the subject of finite intervals, at the same time that he seeks to avoid being responsible for any

one. In speaking of the class of operations for which Mr. Airy has claimed so small a degree of credit, I have, from the generality of his remarks and the impossibility of considering every case, confined myself to those which are best known or are most accessible. It is possible that Mr. Airy may have had in view other investigations than those which I have particularized. If he will take the trouble to specify any such, I will undertake to give them my best consideration and to publish my opinion concerning them, whether favourable or unfavourable; in the latter case supporting my views by argument. But in the mean time I would suggest that, of the cases instanced above, some have been distinctly sanctioned by Mr. Airy himself, and that so lately as the year 1842; and that as his eulogy extends to the whole class of the "mechanical theories," the above must be considered as participating in it. It is a grave charge to make, but until Mr. Airy assures us to the contrary, we must conclude that, at the time of writing the above paragraph, after fair warning, he deliberately entertained the opinion that those unparalleled investigations were of importance "as regards the progress of intellect."

Liverpool, November 25, 1846.

PROFESSOR CHALLIS'S THEORY OF LUMINOUS RAYS.

PROFESSOR Challis appears to have adopted a somewhat novel view of the constitution of light derived from the consideration of the solar spectrum, and which though 'simple' appears to have escaped the notice of all his predecessors.

"If," says he,* "a beam of sunlight pass through a narrow aperture about one-thirtieth of an inch in breadth and be received on a glass prism, the edges of which are parallel to the borders of the aperture, a *spectrum* is formed by the transmitted light, which, when magnified and properly looked at, exhibits, as is well known, a large number of dark lines parallel to the refracting edge of the prism. If instead of passing through an aperture with parallel borders, the light passed through a circular aperture, one-thirtieth of an inch in diameter, a spectrum of diminished width would be seen, but of the same length as before, and crossed by the same dark lines. The transmission through the prism has produced no change in the light: it has only brought into view the parts of which the incident beam is composed. Taking, for instance, a portion of light immediately contiguous to any one of the dark lines, the prism informs us that the incident beam contains light of that particular refrangibility, abruptly terminated in a plane passing through the axis of the beam perpendicular to the edge of the prism. The existence of this abrupt termination is owing to the cause, whatever it may be, which produces the dark line, and has nothing whatever to do with the transmission of light through a small aperture. Let now the prism be turned about the axis of the beam to any other position. The spectrum will present exactly the same appearance as before, and light of the same refrangibility (not necessarily the same light) as in the former case, will still be bounded by a dark line; and so for

* *Camb. Phil. Trans.*, Vol. VIII. part iii. p. 362.

every position the prism be made to take, by being turned about the axis of the incident beam. This experiment proves that every beam of white light contains portions of light of a definite refrangibility, the *sides* of which are turned in all directions from the axis of the beam. This fact is at once explained by supposing light to consist of *rays*: and it does not appear possible to give any other explanation of it. Although the experimental evidence applies immediately only to the portions of light contiguous to dark lines, yet a very strong presumption is afforded by it that all light is in the form of rays. The existence of the dark lines themselves is most simply accounted for by supposing that certain *rays* of sun-light are in some manner extinguished."

This, as an explanation, is not very happy. Professor Challis may know very well what he means when he speaks of "the sides of" a portion of light being "turned in all directions from the axis of the beam," but I apprehend that to most of his readers it will be anything but obvious. Again, one cannot but be struck by the statement that "the transmission through the prism has produced no change in the light," and with another equally round assertion, "that the existence of this abrupt termination has nothing whatever to do with the transmission of light through a small aperture," which every one knows to be untrue, and the Professor as well as the rest, or why did he stipulate for an aperture only one-thirtieth of an inch wide? The rashness of this latter remark, however, may be considered as compensated for by the perfectly unobjectionable character of another which occurs in connexion with it, to wit, that the "abrupt termination is owing to the cause, whatever it may be, which produces the dark line," of which the triteness somewhat verges upon the ridiculous.

To speak seriously however. The broad fact to be accounted for is, that the light in the spectrum terminates abruptly, and this Professor Challis seems to think may be explained "by supposing light to consist of rays," and "that certain rays of sun-light are in some manner extinguished."

No doubt this explanation is perfectly competent as far as it goes, but it is somewhat to be marvelled at how such extinc-

tion as here spoken of takes place. Let us take the case originally proposed by Professor Challis of the narrow aperture, and suppose a lens placed between the prism and the aperture.

A spectral image of the latter may then be received on a screen which, according to the Newtonian theory as to the composition of white light, ought to consist of a series of images of equal breadth, corresponding to rays of different refrangibility, and overlapping each other. But Professor Challis seems to be of opinion that this is not the case. He seems to think that instead of a series of images of nearly equal breadth overlapping each other, the majority of the images capriciously take it upon them to contract themselves to dimensions bearing no proportion to those of the rest: that, for instance, while red rays give an image $\frac{1}{20}$ th of an inch in breadth, blue only give $\frac{1}{100}$ th; the remaining $\frac{4}{100}$ ths which would be due to them upon the Newtonian principle and the laws of plane optics being absolutely "extinguished in some manner."

Has Professor Challis reflected in *what* manner this would be possible? If only a fifth part of the geometrical image due to the blue rays is illuminated, what has become of the rays which should illuminate the remaining portion? If the rays corresponding to one-fifth of the image reach the screen, there can be nothing in the constitution of the prism to prevent those corresponding to the remaining four-fifths from arriving there also. The missing rays cannot have committed suicide by the way. It is very easy to speak of certain rays of sun-light being in some manner extinguished, but unless Professor Challis can throw discredit upon Newton's theory of the composition of white light, or impugn the laws of the formation of images derived from geometrical optics, or shew that the transmissibility of a ray through a glass prism depends upon the length of its course through the prism, and that it is in fact a discontinuous function of that length, it must be admitted that the mode of explanation which he proposes, so far from being 'simple,' presents some of the gravest difficulties that have ever been suggested to the philosophical mind.

It is somewhat remarkable that Professor Challis should have selected the dark lines of the solar spectrum, perhaps the

most obscure branch of the entire subject, as the source from which to illuminate us as to the theory of light. Nor does the particular purpose to which he applies them tend to diminish our surprise at the fact of their being thus introduced to our notice. A plain man, I should imagine, would be rather astonished to be told that philosophers had at length arrived at the conclusion that light consisted of rays, and he might probably be disposed to smile if we were to assure him that this profound result had only been established by means of an experiment which no one ever heard of till within the last fifty years.

Those mystical beings, the dark lines of the solar spectrum, which appear to have taken such hold of the Professor's imagination, are nothing more than the shades of the departed aperture; and a sharp shadow is an apparition which, however difficult to account for, has too long been familiar to us to admit of its being an object of terror. Does Professor Challis think that this difficulty has slept in forgetfulness till the time of Fraunhofer? Suppose the prism removed, and let the naked aperture be then "properly looked at;" shall we not in that case also have a sharply defined shadow? If not, I must beg to suggest that the prism, so far from having "produced no change in the light," has produced a most material change in it.

It is to be regretted that in reintroducing the terms 'beam' of light and 'ray' of light, which have long been dismissed from the service in both the two physical theories, Professor Challis does not define the meaning which he attaches to them. In a subsequent part of his paper (p. 368) he appears to consider a vibration propagated along "a slender cylindrical portion of ether" as equivalent to a ray; but if this be the case, it must be admitted that the fact of the beam being bounded by sharp edges is but meagre evidence of its being composed of a number of distinct threads of vibration. Supposing light to consist of undulations, I apprehend that by a 'beam of light' must be meant an undulation or series of undulations confined between certain planes. Now the agitation of a slender cylindrical portion of ether, of which Professor Challis speaks as if it were what he means by a ray, is simply *an undulation*

confined between certain planes. Thus, according to Professor Challis, the most simple way of accounting for the fact of an undulation being confined between plane limits, is by supposing it to consist of other undulations likewise confined between plane limits, "and it does not appear to him possible to give any other explanation of it." This may be an explanation, but it occurs to me as being of that kind in which nothing is explained.

It is natural that Professor Challis should seek to enhance the difficulty from which he believes his theory to free us. "The facts," he tells us, "are perfectly consistent with the Theory of Emission, and the advocates of that theory might justly appeal to them as evidence in its favour." Yet I apprehend that the most eminent of those advocates, that the illustrious author himself to whom the Theory of Emission owes its every claim to our regard, would have been very little apt to make the appeal which Professor Challis is so ready to allow to his followers. If Professor Challis thinks the abrupt termination in perfect accordance with the Theory of Emission, what does he think of the bearing upon that theory of the fading fringes which occur in diffraction? If the one class of phenomena accord with the Theory of Emission, it will at any rate be by no means easy, it will imply considerations anything but "simple" to reconcile with it the other; and yet upon a conclusion thus hastily adopted Professor Challis would found a new theory of Optics!

The bearing of the phenomena of the pure spectrum upon the constitution of light has been completely misapprehended by Professor Challis. It seems to have escaped his observation that the fusion of colours which occurs when the lens and telescope are removed, is as actual a fact as the accurately defined boundaries which are brought to light by means of those adjuncts. The simple conclusion to be drawn from these two facts taken together is this: that after emerging from the prism, we shall have, corresponding to every point of the aperture and corresponding to every degree of refrangibility of the transmitted light, a diverging pencil of variable intensity, that intensity being greatest in the middle and diminishing

gradually towards the sides. It is not true that, "taking a portion of light immediately contiguous to any one of the dark lines, the prism informs us that the incident beam contains light of that particular refrangibility, abruptly terminated in a plane passing through the axis of the beam perpendicular to the edge of the prism." We might as well conclude from seeing a window-frame cutting sharp against the sky, that the beam which grazes the frame and enters the eye contains "light of that particular refrangibility abruptly terminated in a plane passing through the" edge of the frame and the position of the eye. If vision were confined to the use of the transit instrument, we might indeed arrive at so preposterous a conclusion; but the faculty of locomotion spares us this absurdity.

Satisfied, however, as to the justice of his ray theory upon experimental grounds, and nothing daunted by the unpromising aspect which it presents to the application of the undulatory doctrine, Professor Challis boldly sets to work to shew how the existence of rays may be accounted for on the latter hypothesis. To a practical mechanician such an attempt would seem about as hopeful as the endeavour to make water stand up above the sides of a glass; but to one who has at his command the differential equations of fluid motion which controul the very elements themselves, what shall be impossible?

The Professor sets out from a transformation of the equation of continuity,

$$\frac{d\rho}{dt} + \frac{d\rho V}{ds} + \rho V \left(\frac{1}{R} + \frac{1}{R'} \right) = 0 \dots\dots\dots (1),$$

(in which ρ denotes the density, V the velocity at any point, and R, R' the greatest and least radii of curvature at any point of the surface, whose differential equation is $d\psi = 0$), and the ordinary integral of the general equation of fluid motion,

$$a^2 \log^e \rho + \int \frac{dV}{dt} ds + \frac{V^2}{2} = F(t),$$

"the variation with respect to space being," in these two equations, "from point to point of the line of the motion. By differentiating this equation with respect to s and t successively,

we get

$$\left. \begin{aligned} & \frac{a^2 d\rho}{\rho ds} + \frac{dV}{dt} + V \cdot \frac{dV}{ds} = 0 \\ \text{and} \quad & \frac{a^2 d\rho}{\rho dt} + \int \frac{d^2 V}{dt^2} ds + V \frac{dV}{dt} = F'(t) \end{aligned} \right\} \dots\dots\dots (2).$$

“Hence substituting for $\frac{d\rho}{\rho dt}$ and $\frac{d\rho}{\rho ds}$ from the preceding equations, and differentiating with respect to ρ , the result is

$$\begin{aligned} \frac{d^2 V}{dt^2} - (a^2 - V^2) \frac{d^2 V}{ds^2} + 2V \frac{d^2 V}{ds dt} + 2 \frac{dV}{ds} \cdot \frac{dV}{dt} + 2 \frac{dV}{ds} \left(\frac{1}{R} + \frac{1}{R'} \right) \\ + a^2 V \cdot \left(\frac{1}{R^2} + \frac{1}{R'^2} \right) = 0, \end{aligned}$$

for $dR = dR' = ds$. Now putting $\frac{dq}{ds}$ for V , and integrating with respect to s after the substitution, it will be found that

$$\frac{d^2 q}{dt^2} - \left(a^2 - \frac{dq^2}{ds^2} \right) \frac{d^2 q}{ds^2} + 2 \frac{dq}{ds} \cdot \frac{d^2 q}{ds dt} - a^2 \frac{dq}{ds} \left(\frac{1}{R} + \frac{1}{R'} \right) = F(t) \dots (3).$$

“Lastly, for q put $\phi + \chi(t)$, the function $\chi(t)$ being such that $F(t) - \chi''(t) = 0$.

“Then $\frac{d\phi}{ds} = \frac{dq}{ds} = r$, and

$$\frac{d^2 \phi}{dt^2} - \left(a^2 - \frac{d\phi^2}{ds^2} \right) \frac{d^2 \phi}{ds^2} + 2 \frac{d\phi}{ds} \cdot \frac{d^2 \phi}{ds dt} - a^2 \frac{d\phi}{ds} \left(\frac{1}{R} + \frac{1}{R'} \right) = 0 \dots (4).$$

“If the surface normal to the directions of motion be a plane, R and R' are each infinitely great, and the equation strictly applying to this case of motion is

$$\frac{d^2 \phi}{dt^2} - \left(a^2 - \frac{d\phi^2}{ds^2} \right) \frac{d^2 \phi}{ds^2} + 2 \frac{d\phi}{ds} \cdot \frac{d^2 \phi}{ds dt} = 0 \dots\dots\dots (5).$$

“It is well known that this equation is exactly satisfied by a particular integral applying to motion propagated in a single direction, namely, $\frac{d\phi}{ds} = F \left\{ \left(a + \frac{d\phi}{ds} \right) t - s \right\}$; and that at the same time $\frac{d\phi}{ds} = a \log_e \rho$. From these two equations it follows that a given state of density and velocity is carried through space by the propagation and by the motion of the particles

with the velocity $a + \frac{d\phi}{ds}$. The rate of propagation is therefore strictly a , whatever be the velocity and density of the particles. Unless this were the case, the velocity and arrangement of density in a given wave would change by propagation, however small the motion of the particles might be."

The conclusion here arrived at is somewhat extraordinary. "A given state of density and velocity is carried through space with the velocity $a + \frac{d\phi}{ds}$. The rate of propagation is *therefore* strictly a ." I confess myself unable to apprehend the distinction between "velocity" of propagation and "rate of propagation"; and supposing that no such distinction is intended, I am unable to appreciate the force of the argument that because the rate of propagation is $a + \frac{d\phi}{ds}$ it is *therefore* a . If the

velocity $a + \frac{d\phi}{ds}$ seems absurd or incomprehensible, it may be a reason for rejecting the integral or the theory upon which it is founded, but it cannot justify us in an arbitrary adaptation of results to suit collateral purposes.* But to proceed.

"In order that equation (4), in which R and R' are not supposed to be indefinitely great, may apply to motion in which the type of the wave remains altogether unchanged by propagation, it must be of the same form as equation (5). This will be the case if

$$a^2 \frac{d\phi}{ds} \left(\frac{1}{R} + \frac{1}{R'} \right) = (a'^2 - a^2) \frac{d^2\phi}{ds^2} \dots\dots\dots (6),$$

the resulting equation being the same as (5), with the difference of having a' in the place of a .

Also
$$\frac{d\phi}{ds} = \frac{a^2}{a'} \log_e \rho."$$

Professor Challis next enters into some disquisition to shew that the function ψ , of which he is in search, must contain three arbitrary functions which will enable him to satisfy three assump-

* The velocity of propagation in this case is a and not $a + \frac{d\phi}{ds}$, as may be readily shewn.

tions, which he proceeds to make in order to facilitate his further progress. The three assumptions are, first, "that the propagation of the motion is in a single direction; next, that the motion of the particles, situated in a fixed straight line which I shall call the axis of z , shall be entirely in that line; thirdly, that for the motion along the axis of z the equation (7) is satisfied."

To these is afterwards added a fourth, namely, that the condensation at any point whose coordinates are xyz is $\phi_1(z, t) \times f(xy)$, where $\phi_1(z, t)$ denotes that at any point in the axis of z . Upon this latter basis the Professor then deduces values for u, v, w , namely,

$$u = \phi \frac{df}{dx}, \quad v = \phi \frac{df}{dy}, \quad w = f \frac{d\phi}{dz} \dots\dots\dots (7),$$

and finally arrives at the equation

$$\phi = \frac{m}{n} \cos n(2 - a't) \dots\dots\dots (8),$$

which gives the velocity

$$\frac{d\phi}{dz} = m \sin n(a't - z) \dots\dots\dots (9).$$

"It results from the foregoing reasoning, that if the small vibrations of the æther in the direction of propagation follow the law expressed by the equation last obtained, the condensation in any plane perpendicular to the axis of rectilinear propagation may vary at a given time from point to point, and at the same time the propagation be uniform. A consequence of this result is, that a very slender cylindrical portion of the æther may continue in agitation while the contiguous portions are at rest: and since the law above obtained is that which has been found by experience to apply to the phenomena of light, the existence of *rays* of light, which was proved experimentally at the commencement of this paper, is accounted for theoretically."

The facility with which some writers persuade themselves of the perfect conformity with nature of results deduced from premises which have so very slight a connexion with the facts they are intended to account for, is somewhat remarkable. The above result is entirely illusory.

Since "the first powers only" of the velocities are "taken account of," and since in this case, as Professor Challis assures us (*Camb. Phil. Trans.* Vol. VII. part 3, p. 375),

$$\begin{aligned}\frac{1}{R} + \frac{1}{R'} &= \frac{1}{V} \left(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2} - \frac{dV}{ds} \right) \\ &= \frac{1}{\frac{d\phi}{ds}} \left(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2} - \frac{d^2\phi}{ds^2} \right),\end{aligned}$$

$$\left(\text{since } \frac{d\phi}{ds} = V \right),$$

substituting this value in (4) we have, neglecting the terms depending upon $\left(\frac{d\phi}{ds}\right)^2$ and $\frac{d\phi}{ds} \cdot \frac{d^2\phi}{dsdt}$,

$$\frac{d^2\phi}{dt^2} - a^2 \left(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2} \right) = 0 \dots \dots \dots (10).$$

Had Professor Challis commenced his operations with this equation he would have served every purpose of his investigation, and have saved himself and his readers an infinity of trouble by withdrawing from their attention the ponderous processes depending upon the term $\frac{1}{R} + \frac{1}{R'}$, which are of no use whatever. He would also have avoided the impropriety of expressing by the same symbol ϕ two totally different things, namely at one time the motion at any point, and afterwards merely the motion in the axis of z . To obviate this ambiguity I shall write equations (7), (8), (9), as follows:

$$u = \phi_0 \frac{df}{dx}, \quad v = \phi_0 \frac{df}{dy}, \quad w = f \frac{d\phi_0}{dz},$$

$$\phi_0 = \frac{m}{n} \cos n(z - a't),$$

$$\frac{d\phi_0}{dz} = m \sin n(a't - z).$$

Now, observing that

$$\frac{d\phi}{dx} = u, \quad \frac{d\phi}{dy} = v, \quad \frac{d\phi}{dz} = w,$$

and therefore that

$$\frac{d^2\phi}{dx^2} = \phi_0 \frac{d^2f}{dx^2} \quad \frac{d^2\phi}{dy^2} = \phi_0 \frac{d^2f}{dy^2} \quad \frac{d^2\phi}{dz^2} = f \frac{d^2\phi_0}{dz^2},$$

substituting these values in (10) and eliminating $\frac{d^2\phi}{dt^2}$ by means of (8), we obtain

$$(a'^2 - a^2) n^2 f + a^2 \left(\frac{d^2f}{dx^2} + \frac{d^2f}{dy^2} \right) = 0,$$

$$kn^2 f + \frac{d^2f}{dx^2} + \frac{d^2f}{dy^2} = 0 \dots\dots (11),$$

where

$$\frac{a'^2 - a^2}{a^2} = k.$$

But Professor Challis gives us for the determination of f the following equation,

$$\frac{d^2}{dx^2} \cdot \frac{1}{f} + \frac{d^2}{dy^2} \cdot \frac{1}{f} - kn^2 \cdot \frac{1}{f} = 0,$$

or

$$kn^2 f + \frac{d^2f}{dx^2} + \frac{d^2f}{dy^2} = \frac{2}{f} \left\{ \left(\frac{df}{dx} \right)^2 + \left(\frac{df}{dy} \right)^2 \right\}.$$

Hence, comparing this with (11), we get

$$\left(\frac{df}{dx} \right)^2 + \left(\frac{df}{dy} \right)^2 = 0;$$

therefore

$$\frac{df}{dx} = 0, \quad \frac{df}{dy} = 0,$$

and f = a constant, or the propagation will be the same as in a cylindrical tube where a uniform wave perpendicular to the axis in the direction of that line. So much for the theory of luminous rays.

The theory of rays failing, I conclude that that of polarization will fall with it.

MR. STOKES'S THEORY.

THE Theory of Mr. Stokes, which I now propose to consider, is contained in a paper, "On the Theories of the Internal Friction of Fluids in Motion, and of the Equilibrium and Motion of Elastic Solids," read to the Cambridge Philosophical Society, April 14, 1845, and published in the *Transactions* of that body, (Vol. VIII., part III. p. 287).

Mr. Stokes's Temple of Truth is evidently no Hall of Apollo, in which each object shines resplendent beneath the God of Rays, but rather a dismal cavern in which the trembling votary, scared by unfamiliar forms, is initiated into mysteries which are calculated to bewilder rather than to enlighten. Whatever demand the theories we have hitherto considered may make on our credulity, it must be admitted that they do not tax very heavily the understanding. With Mr. Stokes's theory the case is altogether different. It is only by a painful effort that his reasoning can be understood and his arguments appreciated. Like as in some kinds of magic, the visions of truth are only to be apprehended through the medium of an artificial darkness. It is precisely from the consideration of a state of circumstances which the boldest mind might recoil from as impracticable, that Mr. Stokes professes to elicit the principles of his theory.

The first section of Mr. Stokes's paper commences with an attempt "to define or fix the precise meaning of a few terms" which he has occasion to employ. And first with regard to the expression "the velocity of a fluid at any particular point."

"If we suppose a fluid to be made up of ultimate molecules, it is easy to see that these molecules must, in general, move among one another in an irregular manner, through spaces comparable with the distances between them, when the fluid is in motion. But since there is no doubt that the distance

between the adjacent molecules is quite insensible, we may neglect the irregular part of the velocity, compared with the common velocity with which all the molecules in the neighbourhood of the one considered are moving, or we may consider the mean velocity of the molecules in the neighbourhood of the one considered apart from the velocity due to the irregular motion. It is this regular velocity which I shall understand by the *velocity of a fluid at any point*, and I shall accordingly consider it as varying continuously with the co-ordinates of the point."

If Mr. Stokes finds his idea of the "velocity of a fluid at any point" rendered more precise by the above definition, it is to be feared that his previous notions on the subject must have been very indefinite. We have here in fact two definitions: first, we have a regular and an irregular motion, and we are told that we are to neglect the latter; but we are not informed how we are to distinguish the one from the other: and secondly, we are to take "the mean velocity of the molecules in the neighbourhood of the one considered apart from their irregular motion." Now with regard to this latter definition, which alone appears in any way intelligible, (though in fact just as objectionable as the other), I would observe that, if the velocities of the different molecules in a small geometrical element differ materially, it cannot be of the smallest use to know the value of their mean, since if we had such knowledge it would give us no clue to the actual motion either of the molecule "considered," or of any of its neighbours. Moreover, according to the above definition, the term "the velocity at any point" under such circumstances would have no definite meaning, since the value of the mean velocity would depend upon the size of the element and the mode in which the division into elements takes place, both of which are perfectly arbitrary.* On the other hand, if the variation of velocity is so slight that it may be neglected, why speak of taking the mean of the velocities of the different molecules when that of any one molecule will serve the same purpose?

* A slight change in the origin of coordinates might revolutionize all the averages.

The true principle here to be assumed, and which Mr. Stokes has involved in so much mystery, is this, that *the velocity of the different molecules in any small geometrical element of a fluid may be considered uniform*. This principle I look upon as a characteristic of fluids, as it may be also of rigid bodies when subjected to internal disturbance. It is this principle alone which enables us to pass from the consideration of a fluid as consisting of a series of *separate molecules* divided by intervals, to that of a *continuous medium* in which motion may be referred to pressure. For if this principle do not hold, it is impossible to attach any definite signification to the expression "the pressure at any point," since this expression amounts to nothing more than a mode of speaking of the aggregate effect of a number of contiguous particles; and unless we suppose the effect of each particle to be sensibly the same, the value of this aggregate will depend upon the number and position of the particles we select, and therefore may be made to assume (within certain limits) any assignable value.

No doubt this view of the subject is opposed to Mr. Stokes's idea of the irregular motion of the molecules "through spaces comparable with the distances between them," upon which his whole theory is based: but I would ask, what reason have we to believe in this irregular motion? Has any one seen it? Is it anything else than a gratuitous assumption? It has so long been the fashion to regard a fluid as a collection of highly volatile particles acting upon each other by their mutual attractions and moving in the most independent manner, that the possibility of a quiet-going medium in which the contiguous particles act in some sort of concert, seems to have been entirely overlooked. And yet we have no reason to believe the thing impossible. Rigid bodies are generally considered to consist of particles acting upon each other by their mutual attractions, yet I am not aware that any one considers the particles in *them* to be moving "among one another in an irregular manner through spaces comparable with the distances between them." A fluid after all is something different from a heap of sand. If it be asked how the one differs from the other, I would say that in the one case we have a connexion between the motions of the particles

which does not exist in the other. Nor is this connexion of motion between the particles of a fluid a merely speculative property, but a physical fact of which we have familiar experience in the phenomenon of viscosity, a property which belongs in a greater or less degree to all fluids which can be made the subject of vision, and which may be inferred of those which cannot be so subjected.—But to return.

After giving a definition of relative equilibrium, of which the accuracy may be doubted, but the obscurity is unquestionable, and after illustrating the difference between it and actual equilibrium in a way little calculated to enlighten the student, Mr. Stokes proceeds to state the nature of the forces acting on a small element. These he makes to consist of the external forces (such as gravity) and the pressures arising from the molecular action of the surrounding medium.—It is somewhat remarkable that he here omits to mention the forces which the different molecules of the element itself exert upon each other, more particularly as he considers it an “undoubted result of observation” that such forces “are forces of enormous intensity,” and as in point of fact there is some reason to think that he makes these identical forces play an important part in the ultimate development of his theory. Passing this however, Mr. Stokes next observes that “in consequence of the intensity of the molecular forces,” which he has already told us is “enormous,” “the pressures arising from the molecular action on the element will be very great compared with the external moving forces acting upon it.” He consequently neglects the external forces in all his subsequent operations.

Now it may be observed that it does not necessarily follow that because the molecular forces are enormous the pressures arising from them are so, nor, if the latter were of the magnitude Mr. Stokes supposes, would it follow that the external forces may be neglected in comparison with them, since the motion is affected only by the difference of pressure and not by the absolute pressure.

Waiving this objection however, we next find Mr. Stokes taking for granted that in estimating the pressure at any point we may neglect any motion of rotation about an axis passing

through the particle considered and common to all the contiguous particles, and that we may also neglect any motion of translation common to the whole element. From the latter of these assumptions it follows that if u, v, w , be the velocities parallel to the axes at the point P , whose coordinates are xyz , and $x + x', y + y', z + z'$ are the coordinates of another point P' near to the former, the effect of the motion at P' on the pressure at P will depend on the velocities

$$\frac{du}{dx} x' + \frac{du}{dy} y' + \frac{du}{dz} z' \parallel x,$$

$$\frac{dv}{dx} x' + \frac{dv}{dy} y' + \frac{dv}{dz} z' \parallel y,$$

$$\frac{dw}{dx} x' + \frac{dw}{dy} y' + \frac{dw}{dz} z' \parallel z.$$

These velocities Mr. Stokes shews may be replaced by three linear velocities parallel to the respective axes and an angular velocity common to all the points in the neighbourhood of P . The latter he neglects, and then shews that during a single instant the axes may be so taken that the residual motion about P may be represented by

$$U = e'x, \quad V = e''y, \quad W = e'''z.$$

Without wishing to detract from Mr. Stokes's merit in suggesting this simplification, I may observe that the same result may be obtained somewhat more readily upon a different principle, namely, by assuming that the pressure at any point depends only upon that part of the motion around it which takes place towards or from it, or that that part only of the motion at P' is effectual in producing pressure at P , which takes place along the line joining the two points. For, the whole velocity at P' parallel to this line

$$= \frac{1}{r} \left(\begin{aligned} &\frac{du}{dx} x^2 + \frac{du}{dy} xy + \frac{du}{dz} xz, \\ &+ \frac{dv}{dx} xy + \frac{dv}{dy} y^2 + \frac{dv}{dz} yz, \\ &+ \frac{dw}{dx} xz + \frac{dw}{dy} yz + \frac{dw}{dz} z^2, \end{aligned} \right)$$

which can always be reduced to the form

$$\frac{1}{r} (Ax'^2 + By'^2 + Cz'^2)$$

by transformation of coordinates; and this last expression is evidently the same as would have been produced if we had had three velocities parallel to the new axes

$$Ax', \quad By', \quad Cz'.$$

Mr. Stokes next proceeds to replace the motion represented by the above expressions for U , V , W , by "a motion of dilatation, positive or negative, which is alike in all directions, and two motions which he calls *motions of shifting*, each of the latter being in two dimensions and not affecting the density."

Without entering into any discussion as to the propriety of the latter term, which is not very obvious, I may observe that the motion 'of shifting' parallel to xy consists of a simultaneous contraction parallel to x , and dilatation parallel to y , or *vice versa*, and is represented by the velocities σx , $-\sigma y$, parallel to x and y respectively. The other motion of shifting parallel to xz produces velocities $\sigma'x$, $-\sigma'z$, parallel to x and z . "If δ be the rate of linear extension corresponding to a uniform dilatation," we have

$$\delta = \frac{1}{3}(e' + e'' + e'''), \quad \sigma = \frac{1}{3}(e' + e''' - 2e''), \quad \sigma' = \frac{1}{3}(e' + e'' - 2e''').$$

"Denoting then by $p + p'$, $p + p''$, $p + p'''$, the pressures on planes perpendicular to the axes of xyz , we must have

$$p' = \phi(e', e'', e'''), \quad p'' = \phi(e'', e''', e'), \quad p''' = \phi(e''', e', e''),$$

$\phi(e', e'', e''')$ denoting a function of e' , e'' , e''' , which is symmetrical with respect to the two latter," " p being the pressure which would exist about the point P if the neighbouring molecules were in a state of relative equilibrium." "The question is now to determine, on whatever may seem the most probable hypothesis, the form of the function ϕ ."—To the discussion of this question I must request my reader's particular attention.

"Let us first take the simpler case in which there is no dilatation, and only one motion of shifting, or in which $e'' = -e'$, $e''' = 0$, and let us consider what would take place if the fluid

consisted of smooth molecules acting on each other by actual contact. On this supposition it is clear, considering the magnitude of the pressure acting on the molecules compared with their masses, that they would be sensibly in a position of relative equilibrium, except when the equilibrium of any one of them became impossible from the displacement of the adjoining ones, in which case the molecule in question would start into a new position of equilibrium. This start would cause a corresponding displacement in the molecules immediately about the one which started, and this disturbance would be propagated immediately in all directions, and would soon become insensible. During the continuance of this disturbance, the pressure on a small plane drawn through the element considered would not be the same in all directions, nor normal to the plane: or, which comes to the same, we may suppose a uniform normal pressure p to act, together with a normal pressure p_n , and a tangential force t_n , p_n , and t_n being forces of great intensity but short duration, that is, being of the nature of impulsive forces. As the number of molecules comprised in the element considered has been supposed extremely great, we may take a time τ so short that all summations with respect to such intervals of time may be replaced by integrations, and yet so long that a very great number of starts will take place in it. Consequently we have only to consider the average effect of such starts, and moreover we may without sensible error replace the impulsive forces such as p_n and t_n , which succeed one another with great rapidity, by continuous forces. For planes perpendicular to the axes of extension, these continuous forces will be the normal pressures p' , p'' , p''' .

“Let us now consider a motion of shifting differing from the former only in having e' increased in the ratio of m to 1. Then if we suppose each start completed before the starts which would be sensibly affected by it are begun, it is clear that the same series of starts will take place in the second case as in the first, but at intervals of time which are less in the ratio of 1 to m . Consequently the continuous pressures by which the impulsive actions due to these starts may be replaced must be in the ratio of m to 1. Hence the pressures p' , p'' , p''' , must

be proportional to e' , or we must have

$$p' = Ce', \quad p'' = C'e', \quad p''' = C''e'."$$

Such is the principle upon which Mr. Stokes professes to elucidate the theory of fluid motion,—the “method of starts,”—and certainly a most startling method it is. It is somewhat extraordinary that any person of ordinary judgment should have been deluded into the belief that reasoning of such a nature, which may be correctly characterized as *the argument from the incomprehensible*, can carry with it a particle of conviction to the human mind; and yet a similar kind of argument has been adopted by one far more eminent than Mr. Stokes, and who for upwards of thirty years has been looked up to as the great authority on subjects of this nature.

Mr. Stokes says, “It is clear, considering the magnitude of the pressures acting on the molecules compared with their masses, that they would be sensibly in a position of relative equilibrium except when the equilibrium of any one of them became impossible from the displacement of the adjoining ones.” How does the consideration of the “magnitude of the pressures” enter here? Whether the forces be great or small, it would be very extraordinary if one particle, in the midst of a number of others which are all at rest, should of its own motion start from its position of equilibrium, even assuming it to have all the volatility which Mr. Stokes appears to ascribe to it. Mr. Stokes says, “the molecule in question would start into a new position of equilibrium.” No doubt it would do its best, but it might happen not to find a new position of equilibrium. Does Mr. Stokes really believe that a succession of starts takes place at intervals in the way he describes; or does this method amount to nothing more than dividing the continuous motion of a particle into an indefinite number of short intervals, and considering the separate effect of the motion in each interval? If anything depend on the individuality of the starts, I must beg to state that we have not, nor are ever likely to have, the smallest evidence of their existence: and if Mr. Stokes relies on the division into intervals of the continuous motion, I must be permitted to assert that his *argument* is nothing more nor less than

a flat assumption that the pressures will be proportional to the velocity. Mr. Stokes says, "if we suppose each start completed before the starts which would be sensibly affected by it are begun, it is clear that the same series of starts will take place in the second case as in the first, but at intervals of time which are less in the ratio of $1 : m$." Is this so clear? Undoubtedly the fact may be so, but have we any particular reason to believe that it is? Let us take an analogous case. If I tap with my knuckle against a window pane, all the circumstances occur which Mr. Stokes supposes in his account of the effect of one motion of shifting; the external molecules of the glass "start into new positions of equilibrium," "these starts cause a corresponding displacement in the molecules immediately about those which started, and this disturbance is propagated immediately in all directions, the nature of the displacement however being different in different directions, and soon becoming insensible," &c. &c. Now if I increase the velocity of my hand in the ratio of m to 1, according to Mr. Stokes, the "same series of starts will take place in the second place as in the first, but at intervals of time which are less in the ratio of 1 to m "; and this would be all that would occur. But we know as a matter of fact that the effect in the one case may be nothing more than to cause to "start" some fair tenant of the chamber if the blow be communicated from without, or to "displace" some unfortunate mendicant if administered from within, while in the other—the glass may be broken. We may see, in like manner, that in Mr. Stokes's original case it is not necessarily true that the operation of an acceleration of the velocity of any displaced particle will be simply to shorten the time in which the effects of the displacement will be developed, but, on the contrary, effects of a totally different character may result—in other words it is *not* "clear," nor have we the smallest reason to believe it to be the fact, that the same series of starts will take place in Mr. Stokes's second case as in his first, either at intervals of time which are less in the ratio of 1 to m , or otherwise.

From the above formulæ for p' , p'' , p''' , Mr. Stokes deduces, reasonably enough, the following,

$$p' = -2\mu e', \quad p'' = -2\mu e'', \quad p''' = 0.$$

He then proceeds to consider the effect of a second motion of shifting, which he shews in a manner sufficiently allowable (*i.e.* assuming the validity of his first equations) would give us

$$(A) \quad p' = -2\mu e', \quad p'' = -2\mu e'', \quad p''' = -2\mu e'''.$$

After arriving at these desirable results Mr. Stokes enters into some further disquisitions on the theory of starts, for which I must refer such of my readers as may have any curiosity respecting them to his original paper. He next observes, "there remains yet to be considered the effect of the dilatation. Let us first suppose the dilatation to exist without any shifting: then it is easily seen that the relative motion of the fluid at the point considered is the same in all directions. Consequently the only effect which such a dilatation could have would be to introduce a normal pressure p , alike in all directions, in addition to that due to the action of the molecules supposed to be in a state of relative equilibrium: but since these displacements take place, on an average, indifferently in all directions, it follows that the actions of which p is composed neutralize each other, so that $p = 0$."—This extraordinary conclusion, namely, that "a uniform motion of dilatation, positive or negative, which is alike in all directions," has no effect upon the pressure at the point from which the motion radiates, is more than I was prepared for, even after experience of the strange reasoning in which Mr. Stokes had previously indulged. Mr. Stokes considers the relative pressure at a point to depend upon the relative motion about that point, and yet he proposes to neglect the effect of a general motion of all the surrounding particles towards the point in question! If all the surrounding particles are rushing towards a point with the same velocity, does Mr. Stokes really conceive that they will produce no pressure at the point? Or if all the particles are moving away from a point, does he conceive that such motion will not tend to diminish the pressure at the point? If Mr. Stokes neglects the motion towards or from the point, what motion will he take notice of? Will he rely upon the transverse motion? and if he does, how will he get rid of the angular velocity, the dismissal of which is the leading point in his theory?—The same principle

of the motion of dilatation producing no effect upon the pressure is made use of by Mr. Stokes in another attempt to arrive at expressions for the pressures p' , p'' , p''' . He there says,

"If we had started" (*i.e.* in the general case) "with assuming $\phi(e', e'', e''')$ to be a linear function of e' , e'' , and e''' , avoiding all speculation as to the molecular constitution of a fluid, we should have at once $p' = Ce' + C''(e' + e'')$, since p' is symmetrical with respect to e'' and e''' ; or changing the constants, $p' = \frac{2}{3}\mu(e'' + e''' - 2e') + \kappa(e' + e'' + e''')$. The expressions for p'' and p''' would be obtained by interchanging the requisite quantities. *Of course we may at once put $\kappa = 0$, if we assume that in the case of a uniform motion of dilatation the pressure at any instant depends only on the actual density and temperature at that instant, and not on the rate at which the former changes with the time.*"

Of course we may put $\kappa = 0$ in the case supposed, but it is neither a matter of course nor a matter of fact that we are entitled to make any such assumption as Mr. Stokes here supposes. If we can neglect a uniform motion to or from the point whose state of pressure we are considering, pray what motion may we *not* neglect? If we consider that odd creature of Mr. Stokes's, a motion of shifting parallel to xy , we have the two velocities σx , $-\sigma y$, parallel to x and y respectively; and confining ourselves to the former, we have on the opposite side of y equal and opposite velocities, whence, on Mr. Stokes's principle, they ought to destroy one another and at the same time produce no pressure; so that we might get rid of the motions of shifting just as readily as of the motion of dilatation.

But strange as is the conclusion arrived at by Mr. Stokes, the use he makes of it is still more extraordinary. Having shewn that the motion of dilatation has no effect, an unsophisticated person might have thought that we had for ever done with it, and that the motions of shifting and the equations (A) would have been left in quiet possession of the field.—Far from it.

"If the motion of uniform dilatation coexists with two motions of shifting, I shall suppose, for the same reason as before, that the effects of these different motions are superimposed" (*i.e.* as the effect of the motion of dilatation is nothing, either *nothing* is superimposed on the motions of shifting, or the motions of

shifting are superimposed on nothing): "Hence subtracting δ from each of the three quantities e' , e'' , and e''' , and putting the remainders in the place of e' , e'' , and e''' in equations (A), we have

$$(B) \quad p' = \frac{2}{3} \mu (e'' + e''' - 2e'), \quad p'' = \frac{2}{3} \mu (e''' + e' - 2e''), \\ p''' = \frac{2}{3} \mu (e' + e'' - 2e''')."$$

Of this singular step Mr. Stokes does not give one syllable of explanation. Does it follow, because the motion of dilatation has no effect, that in estimating the effect of the motions of shifting we may substitute $e' - \delta$, $e'' - \delta$, $e''' - \delta$, respectively, for e' , e'' , e''' in the formulæ (A)? Mr. Stokes has such strange notions of logic, that when he writes in this elliptical style it is somewhat difficult to divine how he means to fill up the gap in his argument: but if there be any argument here at all, I imagine it must be something like the following—"As the motion of dilatation produces no effect, it is all one as if there were none; hence we may assume $\delta = 0$."!!! To which it may be replied that, assuming the motion of dilatation to have no direct effect on the pressure, it would nevertheless not be generally true that $\delta = 0$, and it would therefore not be generally true that the quantities $e' - \delta$, $e'' - \delta$, $e''' - \delta$, are identical with e' , e'' , e''' ; and it follows therefore, even on Mr. Stokes's own shewing, that the equations (B) do not generally hold.

I have thought it necessary to discuss in detail the process by which Mr. Stokes arrives at his fundamental formulæ, as I consider that process to be replete with principles of the most dangerous character, and which in fact, if once admitted, must tend to destroy all confidence in the deductions of the human mind. The fallacy of these formulæ however may be shewn in a very direct and simple manner, which, on account of the bewildering tendency of arguments such as I have just been considering, it may be desirable that I should point out.

If e' , e'' , e''' are all positive, and we confine ourselves to the relative motion, the tendency of all the particles in the neighbourhood of that which is being considered must be to fly from it. Now that the retreating of the particles along the axis of x from the proposed particle should have the effect of diminishing the pressure along that axis, as the first of the formulæ (B)

would make it do, is highly reasonable; but that the recession of particles along the axis of y should actually have the contrary effect of *increasing* the pressure parallel to x , as the same formulæ would imply, is a circumstance too incredible to be believed. Upon this ground alone, therefore, I think we are warranted in concluding that this formula is erroneous, and similarly of its two companions. But upon these formulæ Mr. Stokes rests his whole theory of Fluid Motion, and also that of the Motion of Elastic Fluids. It follows therefore that both those theories must fall to the ground.

London, Nov. 5th, 1847.

MR. O'BRIEN'S THEORY.*

THE theory I now propose to consider contains a singular mixture of discrimination and error. Whenever Mr. O'Brien finds the views of preceding writers inimical to his own, he seems to have found little difficulty in demonstrating the absurdity of the former. Nevertheless his own theory is open to objections just as fatal as those of his predecessors. It is not my intention to consider this theory in detail, but I shall content myself with pointing out in it such an amount of fallacy as may serve to shew that the many important results which Mr. O'Brien claims to have discovered do not possess a shadow of value.

Mr. O'Brien forms the general equations of undulatory motion pretty much in the same way as Mr. Kelland. In the simplification of these equations, however, he adopts an entirely different process at the same time that he makes use of the same leading principle, viz. that the displacement of any two particles is very small compared with the distance between them. "This assumption," Mr. O'Brien tells us, so far as he is "aware, has been made by every one who has written on the subject of undulations, whether in the case of light or sound."

Now in *that* theory of Sound which is attributed to Bernoulli, and which is generally looked upon as one of the greatest triumphs of modern analysis as applied to physical science, and as such is not to be lost sight of in the consideration of this matter, I am not aware that any such assumption as Mr. O'Brien here speaks of has been made: nor do I see very clearly how any such could be made having reference to the principle of investigation there adopted—since in that theory

* On the Propagation of Luminous Waves in the Interior of Transparent Bodies. By the Rev. M. O'Brien, M.A., late Fellow of Caius College, Cambridge. *Camb. Phil. Trans.* Vol. vii. part iii. p. 397.

we do not profess to deal with particles separated by intervals, but with geometrical elements in immediate contiguity with each other. To those indeed who, like Mr. O'Brien, look upon it as a certain fact that all matter consists of spherical particles separated by finite intervals, it is possible that even the Bernouillian method implies the truth of some such principle as he here assumes: but I must protest against citing those great writers, to whom we owe everything of value in connexion with this subject, as authorities for assumptions which they never recognised, and in the embarrassment of which they had too much discrimination to involve themselves.

Mr. O'Brien indeed seems to think that "this is no assumption in the present investigation," and gives several reasons in support of this view; amongst others, the distance of the sun from the earth, and the law of the intensity varying as the inverse square of the distance—an argument which might possess some weight if there were no such thing as candlelight. It is suggested to us also that "if this be not true, the principle of the superposition of small motions cannot be applied to the ethereal undulations, and the whole undulatory theory must fall to the ground;" a circumstance which, it may be as well to hint to Mr. O'Brien, would rather operate *against* his assumption than in its favour, in the consideration of some of his readers.

Allowing to pass, however, this and other minor matters, I shall now advert to what I regard as the cardinal error of Mr. O'Brien's theory. Having arrived at the equation

$$\begin{aligned} \frac{1}{m} \frac{d^2 a}{dr^2} &= \frac{M}{2} \left(\frac{d^2 a}{dx^2} + \frac{d^2 \beta}{dy^2} + \frac{d^2 \gamma}{dz^2} \right) + \frac{P}{2} \frac{d^2 a}{dx^2} \\ &+ \frac{N}{2} \left(\frac{d^2 a}{dy^2} + \frac{d^2 a}{dz^2} + 2 \frac{d^2 \beta}{dxdy} + \frac{d^2 y}{dxdz} \right) \end{aligned}$$

+ differential coefficients of the fourth and higher order in which

$$mM = \Sigma m \{ f(r) \delta x^2 \},$$

$$mN = \Sigma m \left\{ \frac{1}{r} f'(r) \delta x^2 \delta y^2 \right\},$$

$$mP = \Sigma m \left\{ \frac{1}{r} f'(r) \delta x^4 \right\},$$

and assuming that " $a\beta\gamma$ will be of some such form as $c \sin \frac{2\pi}{\lambda} (vt - px - qy - sz)$," we are indulged with the following remarks.

"We know that the molecular forces of all ordinary bodies are quite insensible at the smallest distances that can be measured; and therefore they must be so at the distance λ , which, though small, is measurable: hence we may suppose that the particles of ether exercise no sensible force at the distance λ ; and this being the case, $\Sigma f(r) \frac{r^4}{\lambda^4} c$ must be extremely small compared with $\Sigma f(r) \frac{r^2}{\lambda^2} c$, since r is the distance between two particles which exercise a sensible force upon each other."

The contrivances by which different writers on the hypothesis of finite intervals delude themselves into the belief that they have simplified the equations of motion are remarkable, and Mr. O'Brien's contrivance is no exception to the rule—"Because we may neglect the force due to particles whose distance is considerable, *therefore* we may neglect any function of that distance."—Thus, supposing the law of force to be (what, by the way, Mr. O'Brien does not suppose it) that of the inverse square, if we neglect $\Sigma \frac{m}{r^2}$, where r is equal to or greater than λ , according to Mr. O'Brien we may, under the same circumstances, neglect $\Sigma \frac{m}{r^2} \frac{r^2}{\lambda^2}$, $\Sigma \frac{m}{r^2} \frac{r^4}{\lambda^4}$, $\dots \Sigma \frac{m}{r^2} \frac{r^\infty}{\lambda^\infty}$: a conclusion, I must be permitted to suggest, which is so far from obvious, that any theory resting upon it must be entirely rejected.

I think it proper to observe, that in a subsequent part of his paper Mr. O'Brien professes to prove "two remarkable and very general theorems respecting transverse and normal vibrations," which he believes "capable of very important applications"; and which really would be so if they were true, but they in fact are grounded upon an entire misconception of the nature of transversal undulation.

“To shew that the condition of the vibrations being transversal is

$$\frac{da}{dx} + \frac{d\beta}{dy} + \frac{d\gamma}{dz} = 0.$$

“Let the equation to any surface in which all the particles are in the same phase of vibration (*i.e.* any wave surface) be

$$F(xyz) = u,$$

where u is a parameter which does not vary as long as xyz belong to the same wave surface, but is different for different wave surfaces: for example, if the wave surface be spherical we may take u to represent the radius, or if it be plane we may take u to represent the perpendicular upon it from the origin. In the former case the above equation would be

$$x^2 + y^2 + z^2 = u^2,$$

and in the latter $px + qy + sz = u$,

where p, q, s represent the cosines of the angles which the surface makes with the coordinate planes. It is evident that in both these cases u varies only when we pass from one wave surface to another.

“Now, supposing that t is constant, the phase of vibration, or, which is the same thing, a, β, γ , can vary only when u varies; hence a, β, γ must be functions of u and t only, &c.”

It is evident that Mr. O'Brien has not studied the geometrical conditions which govern undulations consisting of transversal vibrations. His definition of a wave surface is evidently this, that it is a surface for every point in which a, β, γ are invariable.

Now let a, b, c be the coordinates of any particle on the surface of a sphere whose equation is

$$a^2 + b^2 + c^2 = r^2;$$

and let $a + x, b + y, c + z$ be the coordinates of any other particle on the same surface. Then if each particle receive the same small transversal displacement, whose resolved parts are represented by $a\beta\gamma$, since each particle will still lie on the surface of the sphere, we shall have

$$(a + a)^2 + (b + \beta)^2 + (c + \gamma)^2 = r^2,$$

$$(a + a + x)^2 + (b + \beta + \gamma)^2 + (c + \gamma + z)^2 = r^2,$$

and we therefore have the equation of condition,

$$x^2 + y^2 + z^2 + 2(a + \alpha)x + 2(b + \beta)y + 2(c + \gamma)z = 0 \dots (1);$$

from which it is evident that all the particles on the surface of the sphere are not capable of receiving the same displacement, but only such as lie in the intersection of the curve represented by the equation (1) with the sphere. We may thus see that it is only in the case of a plane wave that all the particles in the wave surface can receive the same displacement, and Mr. O'Brien's "simplification" therefore cannot be entertained.

In the report of the proceedings of the Cambridge Philosophical Society, contained in the number of the *Philosophical Magazine* for the present month, is inserted a notice of a paper of Mr. O'Brien's recently read to the Society, one of the objects of which is "to shew that the equations of vibratory motion of a crystallised or uncrystallised medium may be obtained in their most general form, and very simply, without making any assumption as to the nature of the molecular forces."

It does not appear whether Mr. O'Brien considers his new theory as an improvement upon or a variation from his former one; and of its merits I must confess myself quite incapable of judging, from the imperfect notice which is contained in the report in question: nevertheless, my conviction of the impossibility of arriving at any practical conclusion by means of a direct attack on the general equations of motion is such, that I am ready to pledge myself to do one of three things—namely, to shew the fallacy of the theory; to shew that it rests upon some unauthorised assumption; or to confess myself in error.*

London, Nov. 21, 1847.

* It appears to have escaped my notice during the cursory examination of Mr. O'Brien's theory, upon which the above criticism was founded, that Mr. O'Brien makes the same false approximation which occurs in Mr. Kelland's first theories, *vide ante*, p. 84. I should otherwise not have entered so fully into the consideration of the former gentleman's paper.

March 24, 1848.

A NEW THEORY OF TRANSVERSAL VIBRATION.

WHATEVER difference of opinion may be entertained with regard to the validity of the processes adopted in the different theories of transversal vibration which have been hitherto proposed, I think there can be none as to their entire failure to familiarize the mind with the peculiar mechanism by which that remarkable kind of motion is produced. With regard to other great mechanical theories the case is different. The general theory of the planetary motions—the mode in which a central force, combined with a transverse velocity, leads to the production of a re-entering orbit—the general theory of sound—the manner in which an agitation of a small portion of the elastic medium may be propagated to the remotest distances—these are ideas which can be rendered familiar to any person of ordinary understanding, without the aid of a line of analysis. But if any person were to attempt, from the existing theories upon the subject, to frame a short explanation of the cause of transversal vibrations, I apprehend he would experience considerable difficulty: and granting that he might be successful in establishing the possibility of that kind of motion, I apprehend that he would wholly fail to convey to the mind that degree of conviction and acquiescence which, in the case of elementary principles, can only result from the simplest and most familiar considerations.

It is precisely to this point that I am desirous of directing the attention of my readers. It is my object to make the idea of transversal vibration as familiar as that of the vibration which occurs in sound; to shew that that species of motion is not so completely *sui generis* as to make it necessary for us, in order to explain it, to change all our ideas respecting

the nature of the ethereal medium, or to have recourse, as the sole means for its elucidation, to doubtful deductions from obscure and insignificant equations. It is my object to shew that, so far from being of a character altogether novel and extraordinary, it is what, upon the occurrence of a very simple modification of the circumstances of direct vibration, would naturally result—to shew that it is not merely what may, but what might be expected to take place; and if in the accomplishment of this aim I shall suggest no considerations but such as are of the simplest character, and such as would require no great stretch of thought to devise, I trust that the results which will be obtained will not be thought altogether devoid of interest or importance.

I conceive that much of the error and confusion of thought which prevail with regard to this subject, have arisen from the non-recognition of the geometrical conditions which govern undulations consisting of transversal vibrations. What is meant by a spheroidal undulation in which the vibrations are transversal? In the case of normal vibrations the definition of a wave is easy: it is a portion of ether which may be divided into concentric laminæ of equal thickness, such that the motion of each small element of any particular lamina is exactly the same.

And in the case of transversal vibrations, if the wave be plane, the same definition will apply; for, however inconceivable the mechanism by which parallel laminæ of ether can be made to move upon one another in a direction transverse to their common normal, there seems to be no geometrical difficulty in the way of our adopting such an idea. But when we come to the case of spherical or spheroidal undulations, the matter is altogether different. It is geometrically impossible to devise a standard according to which the motion in any spherical or spheroidal lamina can be said to be the same throughout.* If we consider the spheroidal wave in uniaxial crystals, in which, according to the received theory, the motion takes place in planes passing through the axis of the spheroid,

* The fallacy of Mr. O'Brien's attempt in this respect has been already pointed out.

a very little reflection will serve to convince us of the truth of this statement. For even if we grant—which, if we consider the medium to be of the same nature as that of sound, would be impossible, and in any case is incredible—that the *amount* of motion of each particle is the same; and if we assume the pole of the spheroid as the standard of direction—which is the most advantageous supposition that can be made—still, for the particles immediately about the pole, even this standard would fail, since, when any particle arrived at the pole, it would be impossible to say what course it should take. And, although it may to some appear of small importance what becomes of a few obscure particles at the pole of the wave, provided the law hold generally, yet, to those who are accustomed to note the admirable exactitude of the operations of nature, the absolute failure of the rule, even in this extreme case, will afford a convincing proof of the necessity for its modification.

Although, however, it is impossible to ascribe to spherical and similar waves, in which the vibrations are transversal, that absolute uniformity of motion which may exist in the case of normal vibrations, we may still assign to them such a distinctive and recognizable character as will enable us to reason with respect to them with clearness and precision. Recurring to the case of the spheroidal wave in uniaxal crystals, we are instructed by the existing theory to consider the vibrations in this case as taking place in planes passing through the axis of the spheroid (*i.e.* along meridians passing through the poles), and no further information is given to us respecting the distribution of the motion along the surface of the wave. If, however, we assume the motion to be symmetrical with respect to the axis, *i.e.* that the motion throughout any section made by a plane perpendicular to the axis is the same; and if we further suppose that all the particles between the equator and the pole are moving in the same direction, *i.e.* that they are *all moving towards* or *all moving from* the pole, a very important consequence will follow.

It is evident that, if all the particles are moving from the equator to the poles, there must be a *suffusion of particles*

upon the latter; or in that part of the wave we shall have condensation, at the same time that the efflux of particles from the equator must occasion a rarefaction at the equator. This state of things evidently can only be temporary. The condensation at the pole will tend to relieve itself by the ether in that part flowing *along the surface of the wave* towards the equator, where the density is less than its own; so that in the end we shall have condensation at the equator where before we had rarefaction, and rarefaction at the pole where before we had condensation. We shall thus have a recurrence of the original motion, the condensation at the equator relieving itself by producing a motion towards the poles, and so repeating the condensation there.—It is this alternate motion from the equator to the pole, and *vice versâ*, which, speaking generally, I consider to constitute transversal vibration.

To descend, however, more into particulars. Suppose our wave to consist of a single condensation, followed by a single rarefaction. It is obvious on this hypothesis that, if the density of each spheroidal element were uniform, there could be none of the lateral motion along the surface of which we have been speaking. For, if we consider the case of a single pulse uniform throughout its whole extent, it is obvious that the effect of the condensed portion of the wave must be, 1st, To flow forwards, so as to produce condensation in front where the density is the mean density: 2nd, To flow backwards, so as to fill up the hollow occasioned by the rarefaction behind it; and this would be the whole motion which would occur.* But, if we make the hypothesis that the intensity of the wave is not uniform throughout, *i.e.* if we suppose that at the pole, for example, the condensed portion is more condensed and the rarefied portion more rarefied than anywhere else, we shall have precisely that kind of motion of which we have been speaking. The condensed portion of the wave, besides the motion in front and rear which it will have, as in the previous case, will tend to flow laterally from the pole towards the equator, since the density in this portion of the wave is

* I consider the above to be the rationale of the undulations which occur in sound.

less at the equator than at the pole; at the same time that, in the rarefied portion of the wave, the effect will be exactly the reverse, since in that portion the density is greater at the equator than at the pole. These effects, however, will by-and-by be inverted. The particles which were at first condensed will, by the progress of the wave, become rarefied, and therefore instead of moving from the pole will tend to move towards that point. Thus, as in the first half of the motion each element has a tendency to move *forwards*, in the direction of transmission, and laterally *away from* the pole, in the second half it will tend to move *backwards* towards the centre, and laterally *towards* the pole. Thus, corresponding to the difference of phase as regards the direct vibration, we have a difference of phase as regards the transversal motion, or a direct and a transversal motion will be communicated simultaneously, will be contemporaneous in duration, and will be propagated together with the same velocity.*

I have above supposed the condensation of the condensed half of the wave, and the rarefaction of the rarefied portion to be respectively greatest at the pole; and further, that in the first half of the wave the condensation diminishes, *cæteris paribus*, as we recede from the pole, and similarly of the rarefaction in the rarefied portion. Now this is equivalent to considering that the *intensity* is a maximum at the pole,

* As the considerations above suggested, though simple, may not be familiar, and as the essence of my theory is comprised in the last few sentences, I must trouble such of my readers as may be desirous of making themselves masters of it, to refer to fig. (7) at the end of this work.

$DdfF$ represents a section of the wave made by a plane passing through the axis AO , $DdeE$ being the condensed, $EefF$ the rarefied portion.

If we have a single wave only, the condensation in $DdeE$ will tend to relieve itself by moving—1st, Forwards *from* the centre, since the ether in front has the density of equilibrium; 2nd, Backwards towards the centre, since the density behind is less than the density of equilibrium, the ether there being rarefied. Now if the density throughout any spheroidal element of the wave be uniform, this is the whole motion which can occur. But if we suppose the density in $DdeE$ to be greatest, *cæteris paribus*, along the line AB , and to diminish gradually as we recede from that line along the surface of the wave towards D and d , besides the motion backwards and forwards which it will have, as in the preceding case, there will likewise be a lateral motion from AB towards DE and de ; and if we further suppose that the rarefaction in $EefF$ is greatest along BC , and diminishes gradually from that line, there will be a motion from EF and ef towards BC ; so that the lateral motions in the two divisions of the wave will be in opposite directions.

and that it diminishes as we recede from that point; and we thus arrive at the important conclusion, that *transversal vibration depends upon variation of intensity in the direct wave*. This variation has no necessary effect upon the velocity of the wave (since waves of all intensities may be propagated with the same velocity), and may therefore occur in spherical waves just as readily as in waves of any other form.

Upon the mode of distribution of the intensity in the secondary waves depends the nature of the polarization, and this distribution will of course vary according to circumstances.

When light is reflected from glass at the polarizing angle, we must suppose the secondary waves (which in this case are spherical) to have their positions of maximum intensity at the extremities of some particular diameter (which may be called the polarizing axis), and that the intensity in each hemisphere diminishes gradually in proportion to the distance from those points respectively. It is easy to see how an arrangement of this kind would give rise to a rectilinear vibratory motion in the direction of great circles of the sphere made by planes passing through the polarizing axis. This axis must evidently be either parallel or perpendicular to the plane of incidence, according as the vibration is supposed to take place in that plane or perpendicular to it. On the latter hypothesis the position of the axis would be absolutely defined, but in the former it would be still indefinite, as almost any position of the plane of incidence would answer the conditions of the problem: nevertheless, to fix our ideas I would suggest that, if the latter hypothesis as to the direction of the vibration be adopted, we should consider the axis to be parallel to the front of the general reflected wave.

When light suffers ordinary refraction, or reflexion otherwise than at the polarizing angle, since in neither of these cases is the vibration rectilinear, it is obvious that the distribution of intensity which we have assumed to obtain in the case last considered will not account for the phenomena; inasmuch as that distribution, as we have already seen, necessarily implies rectilinear vibration. If, however, we assume a *double* distribution of intensity *about two axes at right angles to each*

other, each of which, if it existed separately, would be of the same character as that supposed in the last case, it is obvious that the imperfect polarization which obtains in the present instance may be completely accounted for. It must be observed, however, that although the refracted and reflected waves on this hypothesis, *each undergo, as it were, two simultaneous and opposite polarizations*, these polarizations will be unequal in degree. Thus assuming the polarizing axes in the two waves to be respectively parallel and perpendicular to the plane of incidence, and assuming the vibration in a wave reflected at the polarizing angle to take place in that plane, the polarization of any other reflected wave with respect to the axis in the plane of incidence will be greater than its polarization with respect to the axis in the opposite plane; while with the refracted waves the case will be exactly the reverse: and as the polarization of the reflected wave with respect to the axis in the plane of incidence goes on diminishing as the angle of incidence deviates more and more from the polarizing angle, so the polarization of the refracted wave with respect to the same axis, under the same circumstances, will continually increase; while with regard to the polarization of the two waves respectively in the plane perpendicular to the plane of incidence, the effects will be precisely opposite. Thus taking the two waves together we see that, speaking generally, the amount of action is the same in every direction, or on the whole an *equilibrium of action* is maintained. I shall hereafter take occasion to advert to this circumstance.

If we now turn our attention to the ordinary wave in uniaxal crystals, according to the received theory, we are informed that the motion of each particle in the ordinary (secondary) wave takes place in a section of the sphere made by a plane perpendicular to the axis of the crystal. It is therefore evident, whatever physical theory be adopted, that if this distribution of motion obtains, the mechanical action in the ordinary must be of a totally different character from that which occurs in the extraordinary wave; a circumstance which, so far as I am aware, there is no experimental evidence to warrant us in believing. There would be no difficulty, however, in applying

the principles which I have above suggested to account for the kind of motion under consideration, it being in fact merely requisite for this purpose to suppose that the intensity along any section of the sphere made by a plane *through* the axis of the crystal is uniform. At the same time I confess that I am unwilling to acquiesce in any hypothesis which would recognize a specific difference between the ordinary and extraordinary waves, other than that of form, and I am therefore strongly inclined to regard the polarization of the ordinary as the same in character as that which occurs in the extraordinary wave, and in the waves which arise when light is reflected from glass at the polarizing angle; or, in other words, that it must be attributed to a symmetrical distribution of intensity about a single axis perpendicular to the axis of the extraordinary wave.

A very little consideration, however, will shew that, if this hypothesis be adopted, it will be impossible to consider the axes of both the waves as fixed lines within the crystal; since if they were so, there would be certain directions in which the transmitted (general) waves would be polarized in the same plane; as, for instance, when the direction of the transmitted waves was in the plane passing through the axes of polarization of the two waves. This consideration leads me to infer that the axis of polarization of at least one of the two waves must change its position with the angle of incidence. If only one of the axes was variable, we should naturally conclude that the axis of the spheroidal wave, which, according to the received theory, coincides with the axis of form, would be that which was fixed. I confess, however, that, consistently with the principles above laid down, I am unable to discern any reason why the polarizing axis of the spheroidal wave should be in any way more fixed than that of its spherical companion. For although at first sight it might appear most probable that the spheroidal wave should be polarized with respect to its axis of form; yet if we consider that the form of the wave depends entirely on the velocity, which is absolutely independent of the intensity, to which alone, according to the above theory, the polarization is attributable, it is obvious that, consistently with that theory, there is no reason to expect that the spheroidal wave should

be polarized with respect to its axis of form rather than any other diameter.* Moreover, there would be some difficulty in the application of the above principles if it were supposed that the extraordinary (general) wave could under any circumstances meet its secondary at the extremity of the polarizing axis of the latter. On these several accounts, therefore, I am led to the conclusion, that the polarizing axes of the two waves are alike variable in position according to the direction of the incident light, but that they are always at right angles to each other. In order to fix our ideas I shall make the further assumption that the axes are respectively parallel to the directions of vibration in the general waves.

It will thus be seen that, in opposition to the received theory, I do not conceive double refraction to depend in any degree upon the direction of vibration. I conceive the opposite polarization, which we find to exist in the two waves, to be the consequence, not the cause of double refraction. If by any means two waves are called into play, it appears to me pretty much a matter of course that they should be oppositely polarized. The principle of the equilibrium of action which we have seen to hold in the case of the reflected and refracted rays in ordinary media would lead us to anticipate that such would be the case. How two waves come to be propagated is indeed a dark enigma, and one which I pretend not to unravel. Sometimes it has occurred to me that the ether within the crystal and the molecules of which it is composed act as separate conductors of the incident vibration, which is thus separated into two vibrations propagated with different velocities; while in those bodies which possess only single refraction, either the two waves agree in velocity, or one only is transmitted. One thing however is certain, that the scheme upon which the received theory is conducted of ascribing the variation of velocity to the different elasticity of the medium according to the direction in which the vibrations take place is entirely erroneous, since the occurrence of such a property

* If the polarization be supposed to be with respect to any diameter, the lines of equal intensity will consist of elliptic sections of the spheroid made by planes parallel to the tangent plane at the extremity of the polarizing axis.

within the crystal would produce effects entirely different from those which we find to occur. This may readily be shewn. It is clear that at any moment we shall have within the crystal a series of concentric similar spheroidal laminae or waves diverging from each point on the surface, the *breadth* of any one of which at any point will vary as the diameter passing through that point. Hence the breadth of the m^{th} wave at the equator will be to that of the same wave at the pole, as the equatoreal radius is to the polar radius. But the breadth of the m^{th} spherical wave is the same throughout and equal to the polar breadth of the m^{th} spheroidal wave. Hence, comparing the velocities in the ordinary and extraordinary waves along any diameter perpendicular to the axis of the crystal, it is obvious that, supposing the velocity of transmission to depend solely on the *length of the wave*, and to be *irrespective of the direction of vibration*, the velocities will be actually such as we find them, *i.e.* in the ratio of the greatest and least radii of the spheroid: and it follows that, *if the velocity depended on the direction of vibration, as well as on the length of a wave*, the velocities would *not* be in the ratio of those radii. This conclusion is so obvious that I cannot but feel some surprise at its not having been adverted to. My attention was first directed to it whilst following out to their consequences the principles I have above endeavoured to unfold.

It results, therefore, that the distinctive character of the two waves is impressed upon them *immediately upon entering the crystal*, and that once within it, a wave of a given type will be propagated with a velocity which is quite independent of the direction of its vibration. How two waves are called into play, as I have stated before, I do not profess to explain; but after all, this may be a less anomalous result than is generally supposed. How does it happen that in double refraction each wave is rectilinearly polarized, while in ordinary refraction we have always partial or elliptical polarization? We have seen that the partial polarization which occurs in ordinary refraction may be resolved into two rectilinear polarizations with respect to axes at right angles to each other. Does not this seem to imply that in ordinary as well as in crystalline refraction two

waves in reality exist, which coincide indeed in form and magnitude, but still give indications of their separate origin by the fact of their opposite polarization? It may be considered, to diminish the force of this analogy, that in ordinary refraction the polarization with respect to the two waves is unequal, while in double refraction the two waves are of equal intensity. It may be observed, however, that the inequality of the polarizations in ordinary refraction only exists when the polarization is referred to axes respectively parallel and perpendicular to the plane of incidence. The wave, however, may be split into two of equal intensity though differing in phase, and *polarized with respect to axes lying in a plane perpendicular to the plane of incidence and inclined at angles of $+45^\circ$ and -45° , respectively, to that plane.* If x and y be the coordinates of a particle, where x is measured along the intersection of the plane of the front with the plane of incidence; and x, y , the coordinates of the same particle referred to rectangular axes in the plane of the front inclined at the above angles respectively to the plane of incidence; if

$$x = a \cos \frac{2\pi}{\lambda} (vt - x), \quad y = b \sin \frac{2\pi}{\lambda} (vt - x),$$

we shall have

$$x_1 = \sqrt{2} \cdot \left\{ a \cos \frac{2\pi}{\lambda} (vt - x) - b \sin \frac{2\pi}{\lambda} (vt - x) \right\},$$

$$y_1 = \sqrt{2} \cdot \left\{ a \cos \frac{2\pi}{\lambda} (vt - x) + b \sin \frac{2\pi}{\lambda} (vt - x) \right\},$$

and these last will correspond to two waves of equal intensity consisting of rectilinear transversal vibrations, the polarization being referred to the new axes respectively.*

It will be observed that in the above theory the secondary waves of which I have treated are considered to have an actual existence, and not to be of that ideal character merely which some writers would appear to assign to them. This was the

* In this case, as in those which have gone before, I consider the axes of polarization of the secondary waves to be parallel to the front of the general reflected or refracted wave. The quantities a and b will vary with the angle of incidence.

view of the subject taken by the illustrious founder of the undulatory theory, who conceived the luminous waves to be broken up at every encounter with material particles. It was upon this hypothesis that he established the laws of ordinary reflexion and refraction, and to his modification of the principle as adopted in the two latter cases is to be attributed the greatest triumph of the undulatory theory—the discovery of the law of refraction of the extraordinary wave in uniaxal crystals. I might add that it has been entirely owing to my having proceeded upon that hypothesis that I have arrived at the theory of transversal vibration contained in this paper; respecting which I must be permitted to observe that, whatever opinion may be formed as to whether light is really due to transversal vibrations, or, granting such to be the case, as to whether the transversal vibrations which occur are of the kind I have described, it is the only theory of transversal vibration which has been hitherto proposed, which is in any way intelligible. And lastly, it has been by proceeding upon that hypothesis that I have pointed out for the first time the certain fact that the variation of velocity in the extraordinary wave is not due to its polarization. It will not, therefore, be on slight grounds that I shall give up the actual existence of secondary waves called into play when light is incident upon the surface of a transparent body. I am, however, far from thinking that in applying the principle of secondary waves to account for transversal vibration, considerable modifications of the above theory may not be satisfactorily made, and I am even of opinion that the same mechanical principles may be applied without the aid of the principle of secondary waves.

Thus it may not be necessary to consider the motion throughout each spheroidal lamina of the extraordinary wave to be in the same direction, *i.e.* altogether towards or altogether from the pole. It is sufficient that the direction of motion in each section perpendicular to the axis should be the same; so that we might in fact have a series of annular *transversal undulations* radiating from the pole of the spheroid along the surface of the wave. This idea may be pictured to the mind by supposing a ripple to occur on the surface of a soap bubble, descending

from the highest point, and resembling the ripple which occurs when a stone is thrown into water, with the difference of taking place on a spherical instead of a plane surface. Of course I do not mean that the surface of the wave is elevated and depressed in the case of an ethereal transversal undulation in the same way as the surface of the water, but merely that an alternate condensation and rarefaction takes place in each indefinitely thin lamina *resembling* or *corresponding* to the alternate elevation and depression of the fluid surface.

The fig. (8) represents rudely a section of a wave in which we should have both direct and transversal vibration, without the aid of the machinery of secondary waves; the shaded parts representing the condensations, and the white spaces the rarefactions. It is evident that one effect of the condensations at *A* and *B* must be to fill up the hollow between them at *d*, so as ultimately to produce condensation there, to which purpose the condensation at *D* will also contribute. On the other hand, we shall at the same time have a rarefaction produced at *A* and *B*; and we shall have in like manner a condensation at *e*, and a rarefaction at *C*; so that after the lapse of a certain period the first half of the wave, *AC*, will present the same appearance as the second, *DE*, actually presents. It is easy to see how, under the above circumstances, a transversal oscillatory motion would be maintained and propagated.

It would not be difficult to multiply examples of the application of the above principles. Those which have been already given, however, may suffice. In the foregoing paper it has not been so much my object to propound a settled theory of polarization, as to put the student in possession of a novel and powerful machinery, which I believe to be capable of the most extensive application in the theory of optics.

Liverpool, January 17, 1848.

ON THE SUPPOSED INCOMPATIBILITY OF

TRANSVERSAL VIBRATION WITH THE SONOROUS MEDIUM.

THE substance of the preceding theory was communicated to the Cambridge Philosophical Society at a meeting of that body, which took place in the spring of last year. It was upon the same occasion suggested that the impossibility of transversal vibration in a medium resembling that of sound had been established by Poisson from the consideration of the general equations of fluid motion, and by the kindness of Mr. Hopkins I was referred to several memoirs of that eminent author in which this point is attempted to be proved. Having a pretty strong conviction of the impossibility of deriving any useful result from the general equations of fluid motion, I cannot say that I felt much alarm at such an intimation, and the result has shewn that I had good ground for my confidence.

In the *Journal de l'Ecole Polytechnique*, tome VII. p. 334, M. Poisson commences his attack upon the theory of fluid motion by a transformation to polar coordinates of the well-known equation

$$\frac{d^2\phi}{dr^2} = a^2 \left(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2} \right).$$

If $z = ru$, $x = r\sqrt{1-u^2} \cos \omega$, $y = r\sqrt{1-u^2} \sin \omega$,
this becomes

$$\frac{d^2.r\phi}{dt^2} = a^2 \left\{ \frac{d^2.r\phi}{dr^2} + \frac{d.(1-u^2)}{r^2 du} \frac{d.r\phi}{du} + \frac{1}{r^2(1-u^2)} \frac{d^2.r\phi}{d\omega^2} \right\}.$$

Of this equation M. Poisson obtains an exact integral; but one which, as may readily be surmised, is of no practical utility: although the author appears to have thought that it

enabled him to establish as a general fact the uniformity of the velocity of sound in space. This however, which, so far as I am aware, is the sole result obtained by our author independently of any special assumption as to the nature of the vibration, is erroneous, as will be presently shewn. After attempting to establish this proposition, M. Poisson immediately deserts the general integral, and proceeds to integrate the equation by approximation. The formula he assumes for this purpose is the following:

$$r\phi = f(x, u, \omega) + \frac{1}{r} \int dx f_1(x, u, \omega) + \frac{1}{r^2} \int dx^2 f_2(x, u, \omega) + \&c.,$$

where $x = r - at$; or rather, he takes for the complete value of $r\phi$ the sum of the above series and an exactly similar series in which $x = r + at$: but for the sake of simplicity I shall confine myself to the above, as the consideration of the second series introduces no new feature. He then proceeds to determine the values of the functions $f_1, f_2, \dots$ according to the ordinary principles for ascertaining indeterminate coefficients, and he so arrives at an equation similar to the equation of Laplace's coefficients.

Now it is manifest that the above form of the integral has been suggested by the integral of the ordinary equation for the motion of sound in a cylindrical pipe; and to this mode of integration, considered as a tentative method, no objection can be made; but it is obvious that no very general or decisive conclusions as to the nature of the laws which govern sonorous vibrations can be arrived at in this manner.

M. Poisson next shews that according to the above integral the velocity of propagation will be uniform; and he then proceeds to determine the important point of the direction of the vibration, and it is to this point in particular that I am desirous of directing the attention of my readers.

If m be the angle which the direction of motion makes with the radius vector, we have

$$\cos m = \frac{\frac{d\phi}{dr}}{\sqrt{\left\{ \left(\frac{d\phi}{dr} \right)^2 + \left(\frac{d\phi}{du} \right)^2 \frac{1-u^2}{r^2} + \left(\frac{d\phi}{d\omega} \right)^2 \frac{1}{r^2(1-u^2)} \right\}}}.$$

Now it is evident that the principal term in the above series, which we have assumed as equivalent to $r\phi$, is the first, and that when the distance from the origin is very great, we may in fact put that term to represent $r\phi$; so that we shall have

$$\phi = \frac{1}{r} f(x, u, \omega),$$

and if we neglect powers of $\frac{1}{r}$ above the first, we have

$$\frac{d\phi}{dr} = \frac{1}{r} \frac{df(x, u, \omega)}{dr},$$

$$\frac{d\phi}{du} = \frac{1}{r} \frac{df(x, u, \omega)}{du},$$

$$\frac{d\phi}{d\omega} = \frac{1}{r} \frac{df(x, u, \omega)}{d\omega},$$

from which it appears that $\frac{d\phi}{dr}$, $\frac{d\phi}{du}$, $\frac{d\phi}{d\omega}$ are all of the same order of magnitude. But in the denominator of $\cos m$ the two latter quantities $\frac{d\phi}{du}$, $\frac{d\phi}{d\omega}$ are divided by r^2 ; hence when r is very great these terms will disappear from the denominator, which will reduce itself to $\frac{d\phi}{dr}$, or we shall have $\cos m = 1$, *i.e.* the direction of vibration will coincide with the radius vector. Such is M. Poisson's proof of this important proposition.

The result which M. Poisson has here obtained, naturally flows from the particular form assigned to the integral of the equation of motion; so much so, that it might have been predicated *a priori* that if the integral was of that form, transversal vibration would be impossible.

The integral of the equation is assumed to be

$$r\phi = f\{(r - at), u, \omega\},$$

or, confining ourselves to two dimensions,

$$r\phi = f\{(r - at), u\}.$$

Now it is obvious that if we are to have transversal vibration at all, and at any rate it must be allowed us to begin with,

the above must be modified into some such form as the following:

$$r\phi = f\{(r - at), (u - bt)\}.$$

But if this were the form of the equation, nothing can be more evident than that as the wave advances, *i.e.* as the time increases, the value of $u - bt$ will depend chiefly upon the value of t , since u is confined between the limits $+1$ and -1 , whereas t may have any magnitude whatever; so that, according to this form of the equation, when the distance from the origin is considerable, the variations of u will have little or no effect upon the motion, which will depend entirely on the values of r and t .

To remedy this defect, we must assume the integral of the form

$$r\phi = f\{(r - at), (ru - bt)\}.$$

It will be seen that upon this hypothesis the value of $\frac{dy}{du}$ will no longer be of the same order of magnitude as $\frac{d\phi}{dr}$; for putting

$$r - at = x,$$

$$ru - bt = s,$$

we have

$$\frac{d\phi}{dr} = \frac{1}{r} \frac{df}{dx} + \frac{u}{r} \frac{dF}{ds},$$

$$\frac{d\phi}{du} = \frac{df}{ds}.$$

Hence the denominator of $\cos m$ will no longer reduce itself to $\frac{df}{dr}$ simply, and $\cos(m)$ will have other values than 1, or the direction of vibration will not coincide with the radius vector, but may have any other direction.

Now the form above assumed for $r\phi$ cannot, I apprehend, be readily applied in its present generality to satisfy the general equation of fluid motion; but if we assume

$$r\phi = f(r - at) + F(ru - at),$$

this object will be attained, at least when the distance from the origin is considerable.

The equation of motion in two dimensions reduces to

$$\frac{d^2 r \phi}{dt^2} = a^2 \left\{ \frac{d^2 r \phi}{dr^2} + \frac{1}{r^2} \frac{d(1-u^2)}{du} \frac{d r \phi}{du} \right\};$$

and since the first term in the value of $r\phi$ is known to satisfy the equation, we may omit the consideration of that term and put

$$r\phi = F(ru - bt) = F(s) \text{ suppose,}$$

where

$$s = ru - bt.$$

We have

$$\frac{d^2 r \phi}{dt^2} = \frac{d^2 F}{ds^2} \left(\frac{ds}{dt} \right)^2 = \frac{b^2 d^2 F}{ds^2},$$

$$\frac{d^2 r \phi}{dr^2} = \dots\dots\dots = \frac{u^2 d^2 F}{ds^2},$$

$$\begin{aligned} \frac{d(1-u^2)}{du} \frac{d r \phi}{du} &= -2u \frac{d r \phi}{du} + (1-u^2) \frac{d^2 r \phi}{dr^2} \\ &= -2u \frac{dF}{ds} \frac{ds}{du} + (1-u^2) \frac{d^2 F}{ds^2} \left(\frac{ds}{du} \right)^2 \\ &= -2u \frac{dF}{ds} r + (1-u^2) \frac{d^2 F}{ds^2} r^2. \end{aligned}$$

Substituting these values in the equation of motion, we get

$$a^2 \frac{d^2 F}{ds^2} = a^2 \left\{ u^2 \frac{d^2 F}{ds^2} - \frac{2u}{r} \frac{dF}{ds} + (1-u^2) \frac{d^2 F}{ds^2} \right\},$$

so that the equation will be satisfied provided we neglect $\frac{2u}{r} \frac{dF}{ds}$, which is small in comparison with the other terms when r is large.

I consider, therefore, that I have established my point that transversal vibrations may occur in the sonorous medium even when the distance from the origin is very great.

I believe it will be found that in every instance in which M. Poisson attempts to prove the incompatibility of transversal vibration with the sonorous medium, the form he assigns to his integral is such as to preclude the possibility of that kind of motion when the distance from the origin is considerable.

I now propose to return to the consideration of M. Poisson's proof of the uniform velocity of sound in space. Multiplying all the terms of the transformed equation (1) by du , $d\omega$, and then integrating with respect to u and ω , from $u = 1$ to $u = -1$, and from $\omega = 0$ to $\omega = 2\pi$, we shall have

$$\frac{d^2 rR}{dt^2} = a^2 \frac{d^2 rR}{dr^2} \dots\dots\dots (2),$$

where R denotes $\int \phi \, du \, d\omega$. In fact it is easy to see that the second term of the right-hand member of the equation, when integrated with respect to u , vanishes between the limits $u = 1$, $u = -1$: and the third term vanishes between the limits $\omega = 2\pi$, $\omega = 0$, when integrated with respect to ω .

The integral of (2) is

$$R = \frac{f(r - at)}{r} + \frac{F(r + at)}{r},$$

f and F denoting arbitrary functions. Hence

$$\frac{r^2 dR}{dr} = r f'(r - at) - f(r - at) + r F'(r + at) - F(r + at),$$

$$\frac{r^2 dR}{dt} = ar F'(r + at) - ar f'(r - at).$$

If the velocity of each particle be resolved parallel to its radius vector, $\frac{r^2 dR}{dr}$ will represent the sum of such resolved velocities for all the particles of the sphere whose radius is r ; and $-\frac{r^2}{a^2} \frac{dR}{dt}$ will be the sum of the condensations of the same particles. If then in the origin of the motion it is confined within the sphere of which the radius is a , $\frac{dR}{dr}$ and $\frac{dR}{dt}$ must = 0 when $t = 0$, for all values of $r > a$; which will be the case if Fr and fr vanish for all such values of r . Thus we shall have $f(r - at) = 0$ whenever $r > at + a$ and $F(r + at) = 0$ when $r > a - at$. But, and which is of more consequence than this latter circumstance, we shall have $f(r - at) = 0$ when $r < at$. For if we suppose the original motion to be confined within the spherical shell whose greatest and least radii are a and a'

respectively, it is evident that $\frac{dR}{dr}$ and $\frac{dR}{dt}$ must vanish when $t = 0$ for all values of $r < a'$, therefore $f(r)$ will be zero for the values of $r < a'$ as well as for the values of $r > a$: therefore $f(r - at) = 0$ when $r < at + a'$; and this, however small a' may be; therefore it will be true when $a' = 0$. Hence it is evident that $\frac{dR}{dr}$ and $\frac{dR}{dt}$ will be zero when $r > at + a$; at the instant

when $r = at + a$ they will take other values, and they will again become zero when $r = at$. The molecules therefore at the distance r will remain at rest until $r = at + a$, and they will remain in motion during a time t' such that $a(t + t') = at + a$,

i.e. during a time $= \frac{a}{a}$: they will therefore all move almost at

the same instant, since the duration of their motions will be very short, a being very small with respect to a . Hence, whatever the nature of the original motion, “la vitesse sera la même sur tous les rayons sonores, l’onde sonore conservera toujours une figure sphérique, et la vitesse de cette onde sera constante et égale à a .”

The above demonstration takes for granted the exact point which it has for its object to prove. The quantities $\frac{R dr \phi}{du}$, &c.

are evidently functions of the motion. If therefore the disturbance be discontinuous, unless its form be spherical, we

cannot take the integral of such a function as $\frac{1}{r^2(1-u^2)} \frac{d^2 r \phi}{d\omega^2}$

between the limits $u = +1$, $u = -1$, $\omega = 0$, $\omega = 2\pi$: since that would imply that the motion extended throughout the spherical shell whose radius is r : and therefore, unless we assume the moving mass to be resolvable into spherical concentric elements, the simplification of equation (1), which results from the above method, cannot have place. As some of my readers may be reluctant to believe that so eminent a mathematical writer as Poisson could fall into so decisive an error, and as there may be some vague notion entertained that the above method may be justified upon the principles of discontinuous functions, I shall now proceed to shew how M. Poisson’s method may be made to conduct us to an absurd result.

M. Poisson (p. 337) admits that it is to be feared that the functions $\frac{dR}{dr} \frac{dR}{dt}$ may vanish without the condensations and velocities at the distance r vanishing, which, says he, would destroy the demonstration. "Mais il est evident que cela ne peut avoir lieu qu'en faisant une supposition particulière sur la nature de l'ébranlement primitif, et en plaçant l'origine des coordonnées d'une manière convenable: d'où il suit qu'en transportant cette origine en un autre point de la portion d'air ébranlée, les fonctions $\frac{dR}{dr}$ et $\frac{dR}{dt}$ cesseront de pouvoir être nulles, sans que les vitesses et les condensations le soient aussi." Now as the origin may be assumed without reference to the nature of the disturbance, suppose that the latter is in the first instance confined between two concentric spheres of the respective radii a and a' , where it may occupy either the whole or only a small portion of the intermediate space. It is evident from what has gone before that we shall have $f(r - at) = 0$, except when r lies between $at + a$ and $at + a'$, and we shall have $F(r + at) = 0$ unless r lies between $a - at$ and $a' - at$; from which it is obvious that the whole motion at any instant must be confined between two spherical shells of equal thickness (viz. $a - a'$): of which the outer surface of the one will be at the distance $at + a$, and that of the other at $a - at$ from the origin. As soon as we have $at = a$ this latter will have ceased to exist, and therefore the whole motion will be represented by the function f , and will be confined between the radii $at + a$ and $at + a'$. If therefore the form of the original disturbance be a sphere of the diameter $a - a'$, and a is the radius of a sphere having for its centre the origin of coordinates, and which touches the former at its more remote edge, according to the above theory, after the time $\frac{a}{a}$ has elapsed, we shall have the whole motion comprised between the concentric spherical surfaces whose radii are a and a' respectively: which as a general fact may safely be pronounced to be impossible.

London, April 3, 1848.

REVIEW

OF PROFESSOR CHALLIS'S MORE RECENT SPECULATIONS,
AND OF OTHERS BY THE SAME AUTHOR
NOT PREVIOUSLY COMMENTED UPON.

THE following letter was submitted to Professor Thomson with a view to its insertion in the *Cambridge and Dublin Mathematical Journal*, but that gentleman declined to publish it, for reasons upon which it is unnecessary here to enter. I have no reason to find fault with a decision communicated to me with much courtesy, and resting as I conceive upon fair grounds of support. The letter is given as originally submitted (with the exception of one or two unimportant verbal alterations, and one more material one which I have pointed out); as there did not appear to be any adequate object requiring me to undergo the labour of remodelling it.

To the Editor of the Cambridge and Dublin Mathematical Journal.

SIR,—It is doubtless within the knowledge of many of your readers, that within the last few months some very singular and, if true, very important views have been proposed by the present Plumian Professor of Mathematics at Cambridge, with regard to the Theory of Undulations. They are contained in a series of papers which appeared in the *Philosophical Magazine*, commencing in April last, and for the most part may be classed under the following heads:

1. That undulations may be propagated in thin cylindrical filaments, in the direction of the axis of the filament, without extending themselves laterally.
2. That within such filaments there will be lateral motion, but it will be confined to the limits of the filament.
3. That the velocity of propagation obtained by this theory

will be greater than that ordinarily assigned to the undulations of an elastic fluid.

4. That under no circumstances can there be plane waves or spherical waves.

5. That to obtain true and consistent results in hydrodynamics, another general equation, distinct from those commonly recognized, is absolutely necessary.

Views of such a nature, which profess to supply what has been regarded as the great desideratum of mathematical science for the last thirty years, and at the same time to overthrow every preceding speculation upon the subject, emanating as they do from the successor of Newton, cannot but command attention. Few persons are capable of entering into a critical examination of investigations of this nature: and of the limited number so competent, some of the most able have been too much occupied with theories of their own to give much attention to those of their neighbours, while others have felt little inclined to subject to very refined or searching criticism speculations of whose general object they approved, and of the general ability of whose authors they entertained not the slightest doubt. I hold it to be equally undoubted and lamentable, that, whether owing to these or other causes, a very great amount of error has within the last few years been put forth by some of the most eminent mathematicians of this country with regard to the subjects of molecular action and fluid motion; and this evil, so far from being on the decline, gives every symptom of still further augmenting. Were I to assert that Professor Challis's theory is only one of many which have recently appeared which are equally erroneous, I should simply express my individual conviction as to a matter of fact: but it is impossible, within such limits as a periodical can afford, to substantiate such a position in its fullest extent. I single out, therefore, as the object of my remarks the theory of Professor Challis, partly from the necessity of the case; partly also from its recency, its extraordinary nature, and the conspicuous position of its author. The fact of its having attracted the opposition of the Astronomer Royal in a contest from

which that eminent mathematician is by many considered to have retired with discomfiture, may be thought an additional motive to urge us to a thorough examination of the subject.

Professor Challis starts from the ordinary equations of motion, (*Phil. Mag.* No. 215)

$$a^2 \frac{ds}{dx} + \frac{du}{dt} = 0, \quad a^2 \frac{ds}{dy} + \frac{dv}{dt} = 0, \quad a^2 \frac{ds}{dz} + \frac{dw}{dt} = 0,$$

$$\frac{ds}{dt} + \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0,$$

where s measures the condensation, and u, v, w are the velocities parallel to the axes.

Assuming $-a^2 \int s dt = \phi(xy) \times f(z)$, he thence deduces the following equation,

$$\frac{d^2 \phi}{dt^2} = a^2 \frac{d^2 \phi}{dz^2} + \frac{a^2}{f} \left(\frac{d^2 f}{dx^2} + \frac{d^2 f}{dy^2} \right) \phi,$$

which, by equating the coefficient of ϕ to b^2 , a constant, he splits into two others, viz.

$$(1) \quad \frac{d^2 \phi}{dt^2} - a^2 \frac{d^2 \phi}{dz^2} + b^2 \phi = 0,$$

$$(2) \quad \frac{d^2 f}{dx^2} + \frac{d^2 f}{dy^2} + \frac{b^2}{a^2} f = 0.$$

From the former of these he deduces the integral,

$$\phi = m \cos \frac{2\pi}{\lambda} \left\{ z - at \sqrt{\left(1 + \frac{e\lambda^2}{\pi^2} \right) + c'} \right\},$$

where λ is the length of a wave propagated along the axis of z , and $e = \frac{b^2}{4a^2}$. Professor Challis appears to consider that the

only form of ϕ which "gives a definite solution of the problem" is one consisting of a series of terms similar to the above equivalent for ϕ (see p. 280). It will not be necessary for us to enter into the discussion of this point, but it may be observed, by the way, that this assumption is entirely gratuitous.

"As equation (2) does not contain t , there is no propagation of motion in any direction parallel to the plane of xy : or the

propagation in the direction of z takes place without lateral spreading."

Having arrived at this important conclusion, Professor Challis assumes "that f is a function of r the distance from the axis of z ; according to which the condensation and transverse velocity are the same at the same distance from the axes in all transverse directions. The above supposition respecting f converts equation (2) into the following,

$$\frac{d^2 f}{dr^2} + \frac{df}{r dr} + 4ef = 0,$$

the integral of which in a series is

$$(3) \quad f = 1 - er^2 + \frac{e^2 r^4}{1^2 \cdot 2^2} - \frac{e^3 r^6}{1^2 \cdot 2^2 \cdot 3^2} + \&c.,$$

assuming that $f = 1$ and $\frac{df}{dr} = 0$, when $r = 0$.

"It is easily shewn from this result, that there are an unlimited number of possible values of r for which f vanishes. But we have no right to conclude that (3) gives the expression for f we are seeking for, unless at some [given] distance there be neither condensation nor variation of condensation, for otherwise there will be transverse propagation."

It appears however that, "equation (3) does *not* satisfy this condition," so that instead of equation (2) Professor Challis has recourse to another which he calls an exact equation, of which more hereafter. The rather equivocal proceeding of deducing certain results from exact, and others from approximate equations, was naturally animadverted upon by Mr. Airy (*Phil. Mag.* vol. XXXII. p. 340). I believe however that Professor Challis considers himself to have obviated this objection, by shewing that the adoption of the exact equations throughout will not affect his result. Into this question however it will be unnecessary for us to enter.

"The equation alluded to is

$$\frac{d^2}{dx^2} \cdot \frac{1}{f} + \frac{d^2}{dy^2} \cdot \frac{1}{f} - 4e \frac{1}{f} = 0,$$

from which, assuming f to be a function of r , we obtain

$$(4) \quad f \frac{d^2 f}{dr^2} - 2 \frac{df^2}{dr^2} + \frac{f}{r} \cdot \frac{df}{dr} + 4ef^2 = 0,$$

whence it is clear that if $f = 0$, $\frac{df}{dr}$ also vanishes."

The integral of (4) Professor Challis takes to be

$$(5) \quad \frac{1}{f} = 1 + er^2 + \frac{e^2 r^4}{1^2 \cdot 2^2} + \frac{e^3 r^6}{1^2 \cdot 2^2 \cdot 3^2} + \&c.,$$

whence
$$f = 1 - er^2 + \frac{3e^2 r^4}{4} - \frac{19e^3 r^6}{36} + \&c.$$

"The *least* value of r corresponding to $f = 0$, as given by this last equation, is the radius of a cylindrical surface within which the motion of the fluid *filament* under consideration is contained."

From speaking of the "*least* value of r corresponding to $f = 0$ given by this last equation," it may be inferred that Professor Challis considered that that equation would give more such values of r than one, and it may also be surmised from the general purport of his remarks that this value of r is expected to be very small. It is obvious, however, that the equation $f = 0$, where f is given by equation (5), will have *but one* root, and that root will be $r = \infty$! For it is clear from (5) that $\frac{1}{f}$ can never change its sign ($e = \frac{b^2}{4a^2}$ being necessarily positive), but as r increases from 0 to ∞ , $\frac{1}{f}$ will increase from 1 up to ∞ , and f will *diminish* from 1 to 0, but will *never vanish at any finite distance from the axis of z* , or, to speak more accurately, *will never vanish at all*. So much for the theory of thin filaments.

The absurdity of the entire theory might have been exhibited from an earlier point. If f is a function of r , "the condensation and transverse velocity are the same at the same distance from the axis in all directions." But if the condensation be arranged symmetrically about the axis of z , it is obvious that no force can be acting on the particles in that axis in a direction perpendicular to it; from which it is clear that the motion of those particles must be the same as that in a cylindrical tube when motion is propagated in the direction

of the axis. Hence for particles in the axis of z the equation (1) which represents the motion parallel to z must reduce itself to the ordinary equation of sound, or we must have $b^2 = 0$. Thus the velocity of propagation along the axis of z will *not* be different from that ordinarily given, as Professor Challis supposes. Indeed Professor Challis admits that the result $b^2 = 0$ is entirely incompatible with his theory.

Having thus disposed of the positive part of Professor Challis's theory, I now come to the not less important negative branch of his views, in which he denies, and in fact assumes to disprove, the possibility of plane or spherical waves. When a writer professes to arrive at the conclusion that results are erroneous, which have been known, and as I may say familiarly handled for half a century, without a suspicion of their accuracy, we can hardly repress an involuntary feeling that the zeal of the author has made him the dupe of some egregious fallacy, and in the case under consideration it will be seen that there is good ground for such a sentiment.

In the *Philosophical Magazine*, vol. XXXII. p. 496, we find the following:

"If the motion be parallel to the axis of z , . . . the exact equation applicable to plane waves is

$$(6) \quad \frac{d^2\phi}{dt^2} - \left(a^2 - \frac{d\phi^2}{dz^2} \right) \frac{d^2\phi}{dz^2} + 2 \frac{d\phi}{dz} \cdot \frac{d^2\phi}{dz dt} = 0,$$

which, as is known, is satisfied by the equation

$$(7) \quad w = m \sin \frac{2\pi}{\lambda} \{z - (a + w) t\}.$$

This equation shews that at any time t , we shall have $w = 0$, at points on the axis of z , for which

$$z - (a + w) t_1 = \frac{n\lambda}{2},$$

or
$$z = at_1 + \frac{n\lambda}{2}.$$

At the same time w will have the value $\pm m$ at the points of the axis for which

$$z - (a + m) t_1 = \left(\frac{n}{2} - \frac{1}{4} \right) \lambda,$$

or

$$z = at_1 + \frac{n\lambda}{2} + mt_1 - \frac{\lambda}{4}.$$

“Here it is observable that no relation exists between the points of no velocity, and points of maximum velocity. As m , t_1 , and λ are arbitrary constants, we may even have

$$mt_1 - \frac{\lambda}{4} = 0,$$

in which case the points of no velocity are also points of maximum velocity. This is a manifest absurdity. No step, however, of the reasoning by which this result has been obtained can be controverted. What then is the meaning of it? Clearly the analysis rejects the supposition of plane waves, by giving an integral which admits of no physical interpretation. Plane waves are thus shown to be physically impossible.” So far Professor Challis.

This objection is founded on a mistake. Professor Challis assumes “that at any time t , we shall have $w = 0$ at [all] points in the axis of z for which

$$z - (a + w)t_1 = \frac{n\lambda}{2},$$

or

$$z = at_1 + \frac{n\lambda}{2}.$$

Such is not the case; for if $z_1 - at_1 = \frac{n\lambda}{2}$, it is plain that (7) will give us

$$(8) \quad w = \pm m \sin \frac{2\pi}{\lambda} (wt_1),$$

or

$$\frac{\frac{2\pi}{\lambda} wt_1}{\sin \frac{2\pi}{\lambda} wt_1} = \pm \frac{2\pi}{\lambda} mt_1;$$

and if $w = 0$, this gives us

$$\pm \frac{2\pi}{\lambda} mt_1 = 1,$$

or

$$t_1 = \mp \frac{\lambda}{2m\pi}.$$

Now if, corresponding to the above values z_1 , t_1 of z and t , we have $w = m$, it is clear from (8) that we must have

$$1 = \pm \sin \frac{2\pi}{\lambda} m t_1,$$

and

$$\frac{2\pi}{\lambda} m t_1 = \frac{r\pi}{2},$$

or

$$m t_1 - \frac{r\lambda}{4} = 0;$$

whence we get

$$t_1 = \frac{r\lambda}{4m}.$$

But this value of t_1 cannot be reconciled with that previously obtained, namely $\mp \frac{\lambda}{2m\pi}$, for equating the two, we have

$$\frac{r\lambda}{4m} = \mp \frac{\lambda}{2m\pi},$$

or

$$r\pi = \mp 2,$$

which is impossible, since r is an integer.

Hence it is not true, as asserted by Professor Challis, that when

$$m t_1 - \frac{\lambda}{4} = 0,$$

“the points of no velocity are also points of maximum velocity;” and the objection taken to the theory of plane waves on the assumption of such being the fact, must therefore fall to the ground.*

I now come to the “impossibility” of spherical waves, of which Professor Challis gives us two proofs. If we “suppose the waves to be spherical,” (*Phil. Mag.* vol. xxxii., p. 497) it may be shewn by an integral of the known approximate equation

$$\frac{d^2.r\phi}{dt^2} = a^2 \frac{d^2r\phi}{dr^2},$$

* The above answer to this objection is substituted for one contained in my letter as submitted to Professor Thomson, and which was objected to by Mr. Stokes (to whom the letter was referred by Professor Thomson) as inconclusive.

that the analysis rejects this supposition also. The equation is satisfied if $r\phi = f(r - at)$; whence

$$as = - \frac{d\phi}{adt} = \frac{f'(r - at)}{r}.$$

We may therefore have

$$as = \frac{m}{r} \sin \frac{2\pi}{\lambda} (r - at);$$

and putting $r - at = c$, it follows that a given phase of the wave is carried with the uniform velocity a , the condensation s varying inversely as the distance r . This conclusion is generally adopted: but a very simple application of the principle of constancy of mass will prove that it is false. For if σ_1, σ_2 be the condensations in spherical shells of indefinitely small thickness a , at corresponding parts of the same wave when at different distances r_1, r_2 , the above-named principle requires that $2\pi r_1^2 \sigma_1 a$ should be equal to $2\pi r_2^2 \sigma_2 a$, so that $r_1^2 \sigma_1 = r_2^2 \sigma_2$, and the condensation varies inversely as the *square* of the distance. The inevitable conclusion from this reasoning is, that the analysis rejects the supposition of spherical waves."

Professor Challis's "very simple application of the principle of constancy of mass" is an erroneous application of that principle. The principle of the constancy of mass, as applied to spherical undulations, requires that when a single pulse is propagated of constant thickness, the quantity of matter in an elementary shell at a given distance a from the front of the wave, and *whose undisturbed thickness* is a , should vary inversely as the square of its distance from the origin; but the principle of constancy of mass does not require that the quantity of matter in a shell at a fixed distance from the front, and whose *disturbed* thickness is a , should vary in the above ratio: in other words, the principle does *not* give us the equation

$$2\pi r_1^2 \sigma_1 a = 2\pi r_2^2 \sigma_2 a,$$

as Professor Challis assumes, but the equation

$$2\pi r_1^2 \sigma_1 a_1 = 2\pi r_2^2 \sigma_2 a_2,$$

where a_1, a_2 are the respective thicknesses at given epochs of

the motion of strata at distances r_1 and r_2 from the origin, and whose undisturbed common thickness was a .

The second disproof of spherical waves occurs in the *Phil. Mag.*, vol. XXXIII. p. 463, and stands as follows:

"The pressure p being $a^2(1 + \sigma)$, the following expression is obtained for the condensation σ at the distance r from the centre at the time t ,

$$\sigma = \frac{f(at - r)}{ar};$$

and it is stated that there is no condensation wherever r is greater than $at + \epsilon$ and less than $at - \epsilon$, 2ϵ being the breadth of the sonorous undulation. Hence, supposing 2ϵ very small compared to r , and putting for r outside the function its value at corresponding to the middle of the wave, the quantity of matter existing at any time in the wave beyond what would occupy the same space in the quiescent state of the fluid, is very nearly

$$(13) \quad 4\pi a^2 t^2 \cdot \int \frac{f(at - r) dr}{a^2 t},$$

the integral being taken from $r = at - \epsilon$ to $r = at + \epsilon$. Calling A the constant value of this integral, the expression for the quantity of matter becomes $4\pi A t$. Hence the *matter increases in quantity with the time!* Now the very equations from which this result is derived are founded on the supposition that the quantity of matter is constant. There is consequently no difficulty here which any physical considerations can explain, but strictly a *reductio ad absurdum*, which necessitates the important conclusion that the hypothesis of spherical waves is inadmissible. The physical impossibility of plane waves was proved by the same kind of reasoning; and any attempt to reconcile the contradiction in either case is simply *illogical*. As neither the hypothesis of plane waves nor that of spherical waves is admissible, the theoretical value of the velocity of sound which rests on those hypotheses necessarily fails of support." Professor Challis "returns" therefore "to the consideration of non-divergent waves, or, as they may also be called, *ray-vibrations*."

The conclusion that "the *matter increases* in quantity with the time!" which has excited Professor Challis's note of admiration, is well calculated to do so. It is satisfactory to reflect, however, that this result is not a necessary one, and that spherical waves, equally with their plane companions, may continue to maintain their position in the fields of science undisturbed by any terror of that dread annihilation with which Professor Challis has threatened them.

Upon former occasions Professor Challis has been liberal in making particular assumptions with regard to the form of the function f . We will now indulge him with one.

$$\text{Let} \quad f(at - r) = \sin \frac{2\pi}{\lambda} (r - at),$$

$$\text{then} \quad \int f(at - r) \, dr = \frac{\lambda}{2\pi a} \frac{\cos 2\pi}{\lambda} (r - at),$$

which *vanishes between the limits* $r = at - \epsilon$, $r = at + \epsilon$: so that upon this assumption the expression (13) for "the quantity of matter existing at any time in the wave beyond what would occupy the same space in the quiescent state of the fluid" reduces itself to zero, or the matter does *not* "increase in quantity with the time."

This last remark was communicated to a meeting of the Cambridge Philosophical Society, held on the 11th of December last, upon which occasion Professor Challis stated that "the limits of $r = at - \epsilon$, $r = at + \epsilon$ were not his, they were Mr. Moon's own, and that he (Prof. C.) on that account did not feel called upon to found any observations upon them." This oblivion as to the contents of a paper dated only eighteen days before, occurred to me as rather remarkable, more particularly as I had caused to be printed on slips of paper, circulated among the members of the Society, the different formulæ to which I referred in the course of my remarks, including the statement of the limits of integration. I conclude, however, that Professor Challis's forgetfulness must be attributed to the confusion of thought which is sometimes attendant upon public *viva voce* explanations.

Upon the same occasion the question was suggested by Mr. Adams, "Why a cosine should not be assumed for f as well as a sine?" The answer is, if we assume

$$f(at - r) = \cos \frac{2\pi}{\lambda} (r - at),$$

and consider the initial disturbance to be confined to a sphere whose radius is λ ,* in the beginning of the motion at the distance $r = \lambda$, we should have σ the condensation a maximum, which is contrary to the assumed basis of the investigation, viz. that the variations of density will be continuous.

I now come to Professor Challis's "third general equation in hydrodynamics," which I find somewhat variously stated. In the *Philosophical Magazine*, vol. xxvi. p. 426 (May 1845), this equation is stated to be

$$(9) \quad \frac{d\rho}{dt} + \frac{d.\rho}{dr} V + \rho V \left(\frac{1}{r} + \frac{1}{r'} \right) = 0,$$

where ρ denotes the density, V the velocity, and r and r' the greatest and least radii of curvature of the surface which cuts the direction of motion at right angles. At p. 428 in the paper last referred to, however, "the third general equation" is stated to be

$$(10) \quad \frac{d\psi}{dt} + \frac{d\psi}{dx} u + \frac{d\psi}{dy} v + \frac{d\psi}{dz} w = 0,$$

where $\psi(xyzt) = 0$ is the integral of the equation

$$\frac{u}{\lambda} dx + \frac{v}{\lambda} dy + \frac{w}{\lambda} dz = 0,$$

λ being the integrating factor of the equation

$$u dx + v dy + w dz = 0,$$

it being asserted that (9) may be obtained by combining (10)

* Upon this hypothesis the proper equivalent to be assumed for f would be a discontinuous function which would be equal to

$$\frac{\sin \frac{2\pi}{\lambda} (at - r)}{\cos \frac{2\pi}{\lambda} (at - r)} \text{ from } at - r = 0 \text{ to } at - r = \lambda,$$

and which vanishes for all other values of $r - at$. I have not thought it necessary, however, to adopt this nicety in going through the above reasoning.

with the equation of continuity. Another form of (10) is the following:

$$(11) \frac{d\psi}{dt} + \lambda \left(\frac{d\psi^2}{dx^2} + \frac{d\psi^2}{dy^2} + \frac{d\psi^2}{dz^2} \right) = 0.$$

Now with regard to these different equations it may be remarked—I. that (9) is true, but it is simply a particular deduction from the equation of continuity, and expresses no new mechanical condition whatever; II. that equation (10) is untrue.

As to the first point, Professor Challis gives a general proof of (9) (*Phil. Mag.*, vol. xxvi. p. 425), in which it is evident that the only mechanical condition involved is that of constancy of mass. In the *Cambridge Transactions*, vol. III. p. 373, et seq., he gives a proof of the same equation on the particular hypothesis of $udx + vdy + wdz$ being an exact differential, which is deduced from the equation of continuity by the aid of geometrical considerations only: and the equation so obtained amounts to nothing more than an expression for the radii of greatest and least curvature of the surfaces which cut the direction of motion at right angles (assuming that there will be such surfaces). In the *Phil. Mag.*, vol. xxvi. p. 427, Professor Challis asserts that (9) “cannot be identical with” the equation of continuity, because “in the differential coefficient $\frac{d\rho V}{dr}$ of equation (9) the variation of coordinates is necessarily from one point to another of a line of motion, while the differential coefficients of the equation of continuity are subject to no such limitation.” But admitting this to be an argument against the *identity* of (9) with the equation of continuity, it is none whatever against (9) being a *particular deduction* from the latter. If (9) embodies a new mechanical condition, what is it? Prof. Challis asserts that it embodies the same condition as (10). In vol. xxvi. of the *Phil. Mag.*, p. 428, where Professor Challis states “the third general equation” to be (10), he observes “we might proceed to test the truth of it by employing it with the equation of continuity in the investigation of (9),” and he intimates that he has “already given the mathematical process for that

purpose" in the volume of the *Cambridge Transactions* last referred to, p. 385. Now in this last investigation Professor Challis commences a series of operations of enormous and most needless complexity by finding an expression for

$$\frac{du}{dx} + \frac{du}{dy} + \frac{du}{dz},$$

in the course of which he has recourse to equation (10), or rather its equivalent (11), by means of which he introduces the quantity $\frac{d\phi}{dt}$, which quantity he afterwards eliminates *by means of the same equation*; from which it is obvious that the result of the operations cannot embody the principle of (10). The fact is Professor Challis might have deduced the equation (9) in the general case where $u dx + v dy + w dz$ is only integrable by means of a factor, in precisely the same way as in the *Cambridge Transactions* he deduces it on the hypothesis of $u dx + v dy + w dz$ being a complete derivative, by simply putting

$$\frac{u}{\lambda}, \frac{v}{\lambda}, \frac{w}{\lambda} \text{ for } \frac{d\psi}{dx}, \frac{d\psi}{dy}, \frac{d\psi}{dz},$$

respectively, instead of u, v, w , respectively.

The most complete way of proving, however, that (9) does not embody (10) will be to shew that the latter is untrue. This may be done in a moment.

If $u dx + v dy + w dz$ be an exact differential, we have $\lambda = 1$, and we may put ϕ for ψ , where ϕ is the ordinary auxiliary function of hydrodynamics; (11) will then become

$$(12) \quad \frac{d\phi}{dt} + \frac{d\phi^2}{dx^2} + \frac{d\phi^2}{dy^2} + \frac{d\phi^2}{dz^2} = 0.$$

But when no extraneous forces act, we have

$$(13) \quad \int \frac{dp}{\rho} = -\frac{1}{2} \left(\frac{ds}{dt} \right)^2 - \frac{d\phi}{dt},$$

$$\text{or } a^2 \int \frac{dp}{\rho} = -\frac{1}{2} \left(\frac{ds}{dt} \right)^2 - \frac{d\phi}{dt}.$$

Now the equation of sound is deduced from this last equation and the equation of continuity by neglecting small quantities of the second order; and as we have

$$\left(\frac{ds}{dt}\right)^2 = \frac{d\phi^2}{dx^2} + \frac{d\phi^2}{dy^2} + \frac{d\phi^2}{dz^2},$$

it is clear from (12) that $\frac{d\phi}{dt}$ and $\left(\frac{ds}{dt}\right)^2$ are both of the same order, and may therefore both be neglected. Thus (13) reduces to

$$a^2 \int \frac{d\rho}{\rho} = 0,$$

whence

$$\frac{d\rho}{\rho dt} = 0.$$

But by the equation of continuity

$$\frac{1}{\rho} \frac{d\rho}{dt} + \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0.$$

Hence substituting for $\frac{du}{dx}$, &c., we have

$$\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2} = 0.$$

The true equation of sound however is

$$\left(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2}\right) a^2 = \frac{d^2\phi}{dt^2}.$$

Hence either Professor Challis's equation (9) or the ordinary theory of sound is erroneous.

The above, I think, establishes the fallacy of equation (9): the fallacy of the reasoning by which it is supported may be shewn with equal facility.

“ It is well known that if $\frac{1}{\lambda}$ be a factor which makes

$$u dx + v dy + w dz$$

integrable (*Phil. Mag.* vol. xxvi. p. 427),

$$\frac{u}{\lambda} dx + \frac{v}{\lambda} dy + \frac{w}{\lambda} dz = 0;$$

is the general differential equation of surfaces normal to the directions of motion. Let the integral of this equation be $\psi(xyzt) = 0$. We thus have an equation embracing all such surfaces at all times. It must, however, be observed that the function ψ cannot have this comprehensive character in all cases of motion, unless it may be regarded as an arbitrary or discontinuous function: the same remark applies to the functions that u, v, w are of the coordinates and the time. To represent the velocities at all points and at all times, whatever motion be given to the fluid, they must be arbitrary or discontinuous functions. As however in the investigation of "the ordinary equations of fluid motion" u, v, w were assumed to be functions of constant form for at least indefinitely small variations of the coordinates and the time (for they would not otherwise be subject to mathematical reasoning), the same supposition must be made with respect to the function ψ . Hence $\delta x, \delta y, \delta z$, and δt , being indefinitely small variations, we have for a given form of the function

$$\psi(xyzt) = 0,$$

and $\psi(x + \delta x, y + \delta y, z + \delta z, t + \delta t) = 0;$

whence, by expansion, we readily obtain

$$\frac{d\psi}{dt} \delta t + \frac{d\psi}{dx} \delta x + \frac{d\psi}{dy} \delta y + \frac{d\psi}{dz} \delta z = 0,$$

and putting $\delta x = u\delta t, \quad \delta y = v\delta t, \quad \delta z = w\delta t,$

$$\frac{d\psi}{dt} + \frac{d\psi}{dx} u + \frac{d\psi}{dy} v + \frac{d\psi}{dz} w = 0."$$

The above quotation I have given *in extenso*, though part of the reasoning it contains is irrelevant to the present purpose; the passage however is so obscure that I have thought it desirable to give Professor Challis's *ipsissima verba* upon the subject.

The function ψ in its rudest state will contain an arbitrary function of the time, and one or more arbitrary parameters. Suppose the form of the arbitrary function determined from

the nature of the motion, and let ψ contain a single arbitrary constant a , or let it stand

$$\psi(xyzta) = 0;$$

and let the value of a derived by solution of this equation be

$$a = f(xyzt),$$

the time t being fixed, we may, by assigning successive values to a in the equation $\psi(xyzta) = 0$, obtain all the surfaces normal to the motion at that time. If the surface is to pass through the point x, y, z , at the time t , then for that surface

$$a = f(xyz, t).$$

If the surface is to pass through the point whose coordinates at the time $t + \delta t$, are $x + \delta x, y + \delta y, z + \delta z$, we have a or

$$a + \delta a = f(x + \delta x, y + \delta y, z + \delta z, t + \delta t),$$

from which we get

$$\delta a = f'(t) \delta t + f'(x) \delta x + f'(y) \delta y + f'(z) \delta z,$$

and the second differential of $\psi(xyzta)$ becomes, after equating to zero and eliminating δa ,

$$\begin{aligned} & \psi'(t) \delta t + \psi'(x) \delta x + \psi'(y) \delta y + \psi'(z) \delta z, \\ & = \psi'(a) \{ f'(t) \delta t + f'(x) \delta x + f'(y) \delta y + f'(z) \delta z \}. \end{aligned}$$

Hence putting $xyzt$ for xyz, t , (which last quantities have been introduced simply for the purpose of rendering the reasoning more distinct) and putting

$$\delta x = u \delta t, \quad \delta y = v \delta t, \quad w = w \delta t,$$

we arrive at the equation

$$\begin{aligned} & \frac{d\psi}{dt} + u \frac{d\psi}{dx} + v \frac{d\psi}{dy} + w \frac{d\psi}{dz} \\ & = - \frac{d\psi}{da} \left(\frac{df}{dt} + u \frac{df}{dx} + v \frac{df}{dy} + w \frac{df}{dz} \right), \end{aligned}$$

instead of (10). It remains for Professor Challis to shew upon what hypothesis with regard to the motion, the second member of this equation will vanish.

The considerations I have last suggested are so elementary that I feel almost ashamed of introducing them. When however we find the first recognized mathematical authority in the University of Cambridge overlooking principles so plain, there is no alternative but to shew distinctly their exact bearing.

Professor Challis assumes to shew "the defective state of analytical hydrodynamics when" only the two ordinary equations "are made use of" by an example.

Assuming the fluid to be incompressible, the motion parallel to the plane of xy , and $u dx + v dy$ an exact differential, Professor Challis arrives, by means of the ordinary equations, at the conclusion that "the boundary of the fluid may at *all* times be a cylindrical surface; but it is impossible this can be true, because the particles at the surface are all moving with the same velocity in directions making different angles with the surface." The expression "boundary of the fluid" is somewhat ambiguous. If Professor Challis means by it a solid material boundary, his reasoning does not bear out his conclusion. If he means simply a surface of equal pressure, there is no absurdity in supposing the particles in the cylindrical surface to "be all moving with the same velocity in directions making different angles with the surface." Would Professor Challis have them all moving with the *same* velocity in the *same* direction, and that of the radius? But were the fact otherwise, and the motion indicated by the equation were really impossible, it is still to be observed that the general equations of fluid motion are only approximate (the first of them being founded on the principle of equality of pressure during motion which can only be approximately true), and the question would then arise, whether the alleged absurdity is not owing to our having travelled beyond the limits within which the approximation is feasible.

In the paper in the April No. of the *Phil. Mag.* for the past year, with which I commenced my criticism, Professor Challis "cannot avoid adverting to a difficulty which has long presented itself to him with respect to the explanation usually given to the excess of the velocity of sound above the value a . Admitting that a sudden condensation by developing heat

produces a higher degree of temperature, and therefore of elastic force, than would exist in the same state of density without such development, does it not thence follow, that a sudden rarefaction, by absorbing heat, produces a lower temperature and a less elastic force than would exist in the same state of density without absorption?"

The answer to this question is, that it does *not* follow "that a sudden rarefaction by absorbing heat produces . . . a less elastic force than would exist in the same state of density without absorption"; and for this simple reason, that the effective moving force on any particle does not depend on the *absolute elasticity* of the medium in its neighbourhood, but on the *variation* of elasticity in passing from one to the other of the strata on opposite sides of the particle or lamina whose motion we are considering; which last will increase with a rarefaction just as much as with a condensation, although the former does not.

An answer similar to that just given was afforded by Mr. Airy in his notice of Professor Challis's paper (*Phil. Mag.* vol. XXXII. p. 343), which elicited the following reply. (Vide p. 498 of the same volume.)

"The difficulty respecting the augmentation of the velocity of sound by the development of heat, cannot be so summarily disposed of as Mr. Airy appears to imagine. I shall perhaps succeed better in conveying my meaning by using symbols. If θ be the temperature where the pressure is p and density ρ , and θ_1 the temperature in the quiescent state of the fluid, we have by a known equation,

$$p = a^2 \rho \{1 + a(\theta - \theta_1)\}.$$

Hence

$$\frac{d^2 z}{dt^2} = - \frac{dp}{\rho dz} = - \frac{a^2 d\rho}{\rho dz} - a^2 a (\theta - \theta_1) \frac{d\rho}{\rho dz} - a^2 a \frac{d\theta}{dz} \dots (14).$$

The usual theory explains how the third term of the right-hand side of this equation may be in a given ratio to the first; but my difficulty is to conceive how the same can be the case also with the *second* term, since it changes sign with the change of sign of $\theta - \theta_1$."

Professor Challis here entirely shifts his ground, and instead of elaborating, as he professes to do, his former position, which was untenable, he starts a new difficulty. His first objection to the velocity ordinarily assigned to sound was grounded on the assumption "that a sudden rarefaction would produce a less elastic force" instead of a greater one which is required, and which assumption is not true, as I have shewn. The second "difficulty" is equally fallacious. The assumption upon which "the usual theory" makes "the third term of the right-hand side of the equation in a given ratio to the first," is that the variation of temperature will be proportionable to the variation of density, or that

$$\theta - \theta_1 = k(\rho - \rho_1),$$

ρ_1 being the density corresponding to the temperature θ_1 ; substituting this in (14) the latter becomes

$$\frac{d^2z}{dt^2} = -\frac{a^2}{\rho} \frac{d\rho}{dz} (1 + ak\rho) - a^2ak(\rho - \rho_1),$$

or neglecting the variation of density in the terms depending upon a ,

$$\frac{d^2z}{dt^2} = -\frac{a^2}{\rho} \frac{d\rho}{dz} (1 + ak\rho_1),$$

ρ_1 being the density of equilibrium; the term $a^2ak(\rho - \rho_1)$ vanishing in the approximation. If Professor Challis can understand why we should put

$$-a^2ak \frac{d\rho}{dz} = -a^2ak \frac{\rho_1}{\rho} \frac{d\rho}{dz},$$

I think he may get over his "difficulty" as to "the *second* term" of (14).

My attention having been accidentally directed to Professor Challis's papers, I addressed a short communication to the Cambridge Philosophical Society, which was read at a meeting of that body which took place in November last, in which I pointed out the fallacy of this second "difficulty" as to the velocity of sound, much in the manner I have adopted above. This elicited from Professor Challis the following (*Phil. Mag.* vol. XXXIII. p. 466):

“Let the relation between the pressure and density, *inclusive* of the effect of temperature, be expressed by the equation $p = a^2 \rho^{1+k}$, as is allowable. Then, putting $1 + \sigma$ for ρ , and supposing σ small, we have

$$\begin{aligned} \frac{dp}{dz} &= a^2 (1 + k) (1 + \sigma)^k \frac{d\sigma}{dz} \\ &= a^2 (1 + k) \frac{d\rho}{dz} (1 + k\sigma + \&c.) \end{aligned}$$

If now the terms $k\sigma$, &c. be neglected, the equation is of the same form as that derived from the relation $p = a^2 \rho$, and is consistent with the uniform propagation of a given state of velocity and density. But those terms stand in the way of such a result; and though they are of small amount, yet their effect on the form of the wave must be *accumulative*, and must in the end entirely alter its character.”

With regard to this it may be observed that the effect of the rejected terms “on the form of the wave” is not necessarily “*accumulative*,” but *may* be *periodical*. If however we are to give up the theory of sound on account of an objection such as this, we must equally abandon the Lunar and Planetary Theories. Professor Challis may be prepared to throw overboard the sublime results of his illustrious predecessor with the same composure he has exhibited with regard to the Theory of Sound; but whatever may be his sentiments in this respect, I apprehend that the generality of persons will be still inclined to cling both to the one and the other theory, notwithstanding that “the difficulty alluded to does not exist in the case of ray-vibrations.”

I have now, Sir, brought to a close my observations upon Professor Challis's various speculations. I am aware that your pages have not hitherto been burthened by communications of a controversial character; but at a time when it is almost as easy to publish a mathematical theory as to insert a paragraph in a newspaper, it has appeared to me highly desirable that the crudities which are thus from time to time put forth should be subjected to a more stringent censorship

than any which has hitherto been put in force ; and it has occurred to me that, as conducting the only exclusively Mathematical Journal in England, you might not be unwilling to undertake the responsibility of that office. But you will doubtless form your own judgment as to what is to be the rule of your own conduct, and I have now therefore merely to subscribe myself,

Your obedient Servant,

ROBERT MOON.

Liverpool, Jan. 8, 1849.

THE END





